

# Psychological Bulletin

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# Psychological Bulletin

## THE MEASUREMENT OF ADULT INTELLIGENCE

BY RAYMOND B. CATTELL

*Harvard University*

### PRESENT PRACTICE IN ADULT INTELLIGENCE TESTING

Although the application of intelligence tests to adults—in colleges, industrial concerns, and clinics—has increased prodigiously since the impressive vindication of adult testing in the first World War (135), the present recurrence of an emergency, demanding administration of tests to large numbers of men unused to examinations, has in some respects caught psychologists technically unprepared.

Not that there is any lack of standardized, published tests. A perusal of the *Mental measurements yearbook* (14, 15) and other surveys of mental test material reveals that there are now available, in English-speaking countries, no fewer than 44 intelligence tests either specifically designed for adults or ranging into such levels of difficulty that, although designed primarily for older children, they can also be used with adults.

All varieties of tests find representation in the adult range: verbal literate, verbal oral, nonverbal, perceptual, group, and individual. The following is a fairly complete alphabetical list of available tests:

1. ACE Test for College Freshmen
2. Alexander Performance Scale
3. Arthur Point Scale
4. California Test of Mental Maturity, Advanced Battery
5. Carl Hollow Square Scale
6. Cattell Scale III
7. CAVD Test
8. Chicago Nonverbal Test
9. College Entrance Board of Examiners Test
10. Coöperative Test Service Test
11. Cornell-Coxe Performance Ability Scale
12. Culture Free Intelligence Test, Psychological Corporation
13. Dearborn Anderson Formboards

14. Detroit Advanced Intelligence Test
15. Dominion Group Tests of Intelligence
16. Ferguson Formboards
17. General Intelligence Tests for Africans (Oliver)
18. Henmon-Nelson Test for College Students
19. Herring Revision of the Binet Scale
20. Kent-Shakow Formboard
21. Kuhlmann-Anderson Scale
22. Leiter International Scale
23. McCall, Herring, Loftus Comprehension Battery
24. McGill Picture Anomaly Test
25. National Institute of Industrial Psychology, Test No. 33
26. Ohio State University Psychological Test
27. O'Rourke General Classification Test
28. Otis Self-Administering Test
29. Otis Quick Scoring Test
30. Pintner Advanced Test
31. Porteus Maze Test
32. Pressey Classification Test
33. Progressive Matrices Test (Raven)
34. Psychological Corporation Test VI
35. Psychological Examination, Form D (Teachers College)
36. Revised Alpha Examination, Wells
37. Revised Alpha Examination, Bregman
38. Revised Beta Examination
39. Roback Test for Superior Adults
40. Snedden Disguised Intelligence Test
41. Thurstone Test for Primary Mental Abilities
42. Terman-Merrill Revision of Stanford-Binet Test
43. Tests of General Knowledge (Benge), A Disguised Intelligence Test
44. Wechsler-Bellevue Scale

This list does not claim to be exhaustive, and it deliberately omits highly specialized tests, such as those for conditions of mental deterioration by Babcock(6) and by Simmins (88). Even so, it is at first glance a sufficiently impressive window display; indeed, the adult-testing psychometrist may be led to consider himself richly equipped with 44 tests. Unfortunately, most of them mysteriously melt away when he addresses himself to any specific test program or becomes in the least exacting about technical requirements. Thus, the requirements of a group-testing situation reduce the available tests to about 32. Twenty-eight, being verbal, are unsuitable for groups containing individuals of foreign extraction or very uneven educational background. Eight are non-verbal, but may be said to jump out of the frying pan into the fire by making equally great demands on that purely manipulative



dexterity which enters into performance tests. Only six or seven can be considered nonverbal and nonmanipulative, but not all of these are purely "perceptual," *i.e.* free from pictorial, representative apperception.

Furthermore, a considerable proportion of these tests, in difficulty, style, and educational presuppositions, have every evidence of having been designed for college students, so that they are of uncertain validity and even inadequate standardization when applied to the general population. Fewer than five of the tests have any provision, either in the published norms or in associated research data, permitting an allowance in the calculation of adult mental level for the downward trend in average performance which occurs in timed tests for adults beyond 20 years of age.

Experience of applying the present adult tests to Army and Navy recruits is still too recent to have issued in any quotable, authoritative, psychological literature. But the writer has letters from two psychologists in England, where such testing began earlier, expressing surprise and some dismay as a result of applying well-tried and familiar adult college student tests to averagely gifted and decidedly older recruits. The style of the tests, the form of the instruction, and the type of knowledge or skill assumed were found by psychologists having clinical experience to be inappropriate and not conducive to fair measurement of abilities as judged by other criteria.

In this country the use of adult tests with recruits is now being energetically approached by several relatively independent groups of psychological workers and by a comparatively "clinical" process of trial and error which, in a year or so, may be expected to yield valuable observations (3, 46, 68). A few comments are already published. Thus Atwell, Bloomberg, and Wells (5), reporting on the practice at a New England army induction center, state:

It was originally believed that the verbal items of the Wechsler-Bellevue Intelligence Scale, supplemented by a vocabulary test and alternate arithmetic questions from the Alpha test, would be satisfactory for those cases in which verbal tests could be justified. The selection of non-verbal tests caused more difficulty, but it was expected that the Beta Block Counting test, the eleven, twelve and fourteen year levels of the Porteus mazes, and the third, fourth and sixth designs of the Wechsler-Bellevue Block Design test would be relatively useful.

The lack of non-verbal material that would be adequate and not time consuming was keenly felt, not only with selectees with foreign backgrounds, but also with many who lacked skill with their own language.

An equal sense of dearth has been felt by other workers with noncollege adults. Thus, Weisenburg, Roe, and McBride (129) preface their 1936 study of adult intelligence with the observation that, "while some few intelligence tests for adults exist, notably the Army Alpha and the various examinations for high school or college students, none was sufficiently comprehensive or sufficiently analytical. . . . In any case, all the tests for college students could immediately be eliminated as too difficult for the average adult." A good deal of the military personnel work at the moment seems to be carried out by means of a general classification test, substituted for Army Alpha, and containing "arithmetic, box counting, and vocabulary items, to represent numerical, reasoning, spatial and verbal abilities" (46); while the Navy directive indicates the use of the Kent EGY, Porteus Maze, and Bellevue-Wechsler, with various additions.

The present dearth of tests must come as a shock to most psychologists, for it has been widely assumed that the momentum of real progress in intelligence test theory and practice which arose in the first two decades of this century has continued unabated through the ensuing 20 years. Indeed, most psychologists not actually engaged in research in the field are given to thinking of intelligence testing as a comparatively closed issue. A sharp medicine for this illusion is provided first by the dilemma of the practicing psychologist when he casts around for actual tests suitable for adults in the present emergency. He finds himself, indeed, scarcely better equipped than Yerkes and his co-workers (135) in the memorable enterprise of 1917. It is but a few years, in fact, since Wells (130), with unrivaled experience of the situation, remarked that the reappearance of the Army Alpha "a decade and a half (after its first use) may find the informed reader somewhat puzzled as to the reason for its rejuvenation. The reasons center around the considerations that for all its technical defects it is still the most widely standardized of adult 'intelligence' tests."

But the illusion is even more painfully shattered by the further discovery that the inadequacy which appears in adult intelligence testing is only a particularly obvious outcropping of a weakness that runs through the whole structure of intelligence test practice. This weakness, in fact, goes back to the fundamental theory itself.

The most natural assumption, on first glancing at the problem, would be that the progress of adult intelligence testing has been at a standstill because of certain difficulties peculiar to the testing

of adults. Certainly a brief acquaintance will show that many technical obstacles have to be overcome which do not appear in the testing of children and which probably exceed even the special difficulties encountered in testing infants. Chief among these are:

(1) The greater need for freeing adult tests from assumptions of uniform knowledge, education, and skills. This arises from greater variability among adults in their remoteness from schooling and from specialization of interests owing to occupational concentration.

(2) The comparative difficulty of achieving a test standardization based on really adequate sampling and the consequent uncertainty regarding the true mean and standard deviation of adult scores.

(3) The unsolved problem of devising a means of expressing scores in comparable and meaningful units, usable for all purposes. In this is involved the problem of fixing the denominator of chronological age to be employed in calculating adult intelligence quotients.

(4) The difficulty created by the age decline of test scores on speeded (timed) intelligence tests among adults. This leads to new problems of standardization and raises questions about the nature of intelligence.

(5) The lack of success which has met all efforts to discover intelligence subtests having a validity and predictive value for adults as high as those obtained for the subtest varieties used with children.

Data and viewpoints on each of these problems of adult testing will be reviewed later in this article, but it is necessary to precede such discussions by facing the fundamental problem we have already mentioned, the problem which adult testing shares with all intelligence testing—namely, that concerning the nature of intelligence. For every one of the above problems, in any case, goes back to this basic question. If we begin eliminating schooling and particularly verbal and number skills from adult intelligence tests, we are quickly pulled up by those who insist that these abstract operations are the very essence of intelligence. When we debate the scatter of adult intelligence scores and the true IQ means for various occupations, we immediately encounter the fact that some intelligence tests, particularly those consisting largely of perceptual subtests, give a much greater standard deviation than we are accustomed to find in Binet tests. The accepted scatter and even the degree of constancy of the intelligence quotient depend on the choice of subtests to be accepted as intelligence tests. The age decline of intelligence in "speed" but not in "power" at once provokes a controversy on the meaning of intelligence. Discussions of validity and predictive power of adult subtests naturally lead immediately to the same issue and so on through various aspects of adult testing.

In short, the present status of adult intelligence testing cannot be meaningfully discussed without a considerable digression on theories as to the nature of intelligence. It is the absence of such discussion and the assumption that this field of research is a closed book which are almost entirely responsible for the present chaos in adult intelligence testing. That this chaos cannot in practice be so easily hidden as in the less exacting field of child testing is perhaps fortunate for the progress of knowledge in mental testing.

### *Current Statements on the Nature of Adult Intelligence*

A simile adequate to illuminate the present incredible state of affairs in intelligence theory is not easy to find. It is as if one came prepared to view a museum of prehistoric animals and awoke to a nightmare realization that the monsters were alive and loose. Every definition of intelligence that roamed the textbooks and articles of 1910 (80) will be found flaunting itself openly or lurking dangerously implicit in the technical literature of 1941. Mostly, however, these superannuated concepts have to be dragged into the light, often to the embarrassment of those in whose work they have been implicit. For it has recently become fashionable to be exacting about "test validation" while shunning all reference to the nature of intelligence itself. One may sympathize with a conversational taboo on so unpleasantly derelict a subject!

However, rather than study what is actually done by test constructors, we will first deal with explicit statements, in so far as they are obtainable, for it would be unfair to judge aspiration by actual performance. In a field beset with such practical difficulties the test designer is entitled to expect from the onlookers the old indulgence demanded in "Do what I say, not what I do."

At once, in reviewing the recent literature, one is struck by the prevalence of most of the classical definitions of a generation ago.

One finds Binet's "intelligence is judgment, or common sense, initiative, the ability to adapt oneself." Then various equivalents of Burt's "innate, all-round mental ability" (18). Terman's "ability to think in terms of abstract ideas" has numerous unconscious imitators (101). Also one finds Woodrow's "the capacity to acquire capacity," and Thorndike's "the power of good responses from the point of view of truth." But many of the modifications and elaborations of these propositions cannot be considered improvements. Theory has gyrated rather than progressed. Is Henmon's "Intelligence is capacity for knowledge and the knowledge possessed" (51) any advance on Woodrow's neat "capacity to acquire capacity," or Freeman's "functional adaptation" (37) any

improvement on the accuracy of his earlier concept? (36) Because of the many unblushing reinstatements of once-abandoned errors, one cannot avoid the conclusion that some of the recent clinical authorities are innocent of the original derivations of their ideas.

Broadly viewed, the more valuable and less obviously fallacious of the definitions and obiter dicta today fall precisely under the same three general headings as could be found 20 years ago, representing the best thought of three different fields: (1) clinical study of the defective, (2) biological observation and animal experimentation, and (3) educational measurement. These respective representative definitions issuing from these fields are; (1) the capacity to think abstractly, (2) the ability to learn, and (3) the capacity to adapt means to ends. Apart from mere circular definitions which use the expression efficiency and define ability tautologically as efficiency of performance, or vice versa, all important definitions seem classifiable in these categories.

Wechsler, in his *Bellevue Scale manual* (127), defines adult intelligence as "the aggregate or global capacity of the individual to act purposefully, to think rationally, and to deal effectively with his environment." But Wells (15, p. 265), deploring the fact that Wechsler's concept of intelligence involves adding up the subtest scores into a single total, says: "The chief use of global scores is administrative."

Pintner evidently takes the view that intelligence is what has been measured by past intelligence tests, for he validates his tests by correlating them with other well-known intelligence tests; Bellows (15, p. 238), in his evaluation of the Pintner tests, readily accepts this as a satisfactory proof of their measuring intelligence. This practice is widespread, and its theory is often explicitly stated.

A number of psychologists have discovered that it is rewarding to say boldly what intelligence is not, while being extremely cautious of saying what it is. Thus Keating, in a review of Alexander's *Passalong test*, says (15, p. 237): "The test correlates low with the Stanford-Binet ( $r = .21$ ), a point perhaps in its favor as a non-verbal test; . . ." The argument runs that, other things being equal, a test of nonverbal intelligence is one which avoids measuring what verbal intelligence tests measure. Perhaps Keating is himself displaying a too exclusively verbal intelligence in this syllogism, but it is at least widely accepted among clinical psychometrists. An equally curious logic of mutual exclusion is found in Wechsler's view that the revised Army Beta test should not be considered a substitute for a performance test of intelligence because the tests "involve functions which measure very much the same things that verbal tests do, except that the raw data furnished the subject are given in pictures and symbols and not in words" (15, p. 242).

Many designers follow the practice expressed by Amoss in the manual for the Ontario School Ability examination (137), which aims at "the



elimination of all special skills," and some stipulate specifically that "performance" skills should be avoided. Conrad says that "performance tests do not, to date, correlate sufficiently well with mental tests to serve as an adequate substitute," but adds that in adult tests "language and vocabulary factors should not be overweighted" and "differences of ability or facility in reading and writing, and differences in sensorimotor speed or acuity should have a minimal effect" (op. cit., p. 77). Porteus would also, curiously enough, rule out nonverbal abilities from his concept of intelligence as measured by the ordinary intelligence test, for he remarks: "I doubt very much if we can ever substitute a non-verbal for a verbal test any more than we can substitute a musical test for one in mathematics."

The most prevalent of all viewpoints, among those who venture to express positive statements about the nature of intelligence, is no different from that found among the first experimenters with tests 40 years ago. It is implied in the notion that intelligence is ✓ to be measured by sampling as many abilities as possible.

This view is sponsored by Kuhlmann when he criticizes the Otis test because its subtests (15, p. 235) are "lacking in variety, resulting in the reduction of the number of abilities measured by the battery." On the delicate, not to say crucial, issue of what tests are to be thrown into the pool, Kuhlmann, in the manual for the Kuhlmann-Anderson tests, remarks: "We have depended on common sense analysis in the selection of tests." An equally arbitrary pooling procedure is found in the ACE test, in which it is considered desirable to divide the test into two "kinds of ability," linguistic and quantitative, and to weight the first to double the second to give a final measure of mental capacity. The California tests (98) logically extend their sampling (since there is clearly nothing to forbid it in this general theory) to tests of visual acuity, hearing, motor coordination, and memory.

Although the "sampling" view is often adopted out of naïveté regarding theoretical issues, or from a failure to see any need for further progress, it is also found among psychologists of great experience, sophistication, and theoretical acumen. Thorndike states it in a more developed form with regard to the CAVD test (112), which, however, by yielding three distinct scores, indicates a belief that sheer sampling is not enough. Thus, there is (1) a measure of level of achievement or altitude, measured by the level at which the person succeeds in half the items, (2) a measure of width at a particular level, *i.e.* the per cent of items passed at a given level, and (3) area, the sum of successes at all levels, which corresponds most nearly to a sampling measure. The fact that Thorndike found width to increase quite rapidly during adolescence and level very slowly would seem to indicate that level is most nearly the same as that which other intelligence tests are measuring. Thorndike's choice of subtests, however, is based on clearly stated general principles—namely, such reasonable assumptions as that "responding to parts, elements or aspects of situations is more intellectual than responding to gross total situations".



and that "responding to relations between objects is more intellectual than responding to objects" (112, p. 63).

Garrett seems to be representative of those who accept the sampling process with reservations, for, in commenting on the Kuhlmann-Anderson procedure of adding up different types of test performance, he states that, "since the abilities of school children are relatively undifferentiated as compared with abilities of adults, this is probably as satisfactory a method as any" (15, p. 226). Here, incidentally, is an implicit recognition that the intelligence testing of adults is more exacting with regard to technicalities of test construction and clarity of theory. Finally, one may remind the reader that a sampling theory is very explicitly worked out in the factorial theory of Thompson (106) and was, at least until recently, embodied in the consistent theory and practice of Burt (17, 18).

It is symptomatic of the general muddle that those applied psychologists who have been the most prolific designers and users of "intelligence tests" and who have so long and so uncritically accepted the sum of a hodgepodge of tests in the form of a single IQ measurement are now swinging—on no better evidence—to the opposite extreme of demanding that tests should yield measurements of separate abilities. They wish to proceed to measure special or restricted abilities without first having solved, or shown any means of solving, the problem of the existence and nature of a general ability.

Discussion among clinical and guidance psychologists during the last 20 years shows this unmistakable trend towards interest in measuring special abilities, though not all of it proceeds as if the problem of measuring general ability were settled. Among academic psychologists there has developed simultaneously a critical attitude to the global score and an emphasis on the need for measuring independent abilities.

Thus, Anastasi, in a review of the Terman group test (15, p. 250), criticizes it on the ground that, "for any problems requiring fine discrimination," a test which "attempts to measure in a single score such a composite and ill-defined characteristic as 'general intelligence' is unsuitable." She adds that "undoubtedly tests of separate aptitudes will gradually replace the general intelligence tests for this purpose." One wonders exactly what purpose is envisaged. Is it possible to predict performance in any special field from a measure of special aptitudes apart from general intelligence? And what are these special aptitudes or traits that are so accurately defined?

Elsewhere (2) Anastasi writes that "the measurement of 'general intelligence' is losing its prominence as a goal of testing. Individual as well as group differences are being sought on narrower and more clearly definable traits." If this is true—and our survey indicates that it is—it

simply shows that applied psychologists are trying to run before they have learned to walk.

One immediate result of this drift of opinion has been the production of tests claiming to measure special aptitudes. But the very people who instigated this trend in procedure have been the first to raise an outcry against the tests which have appeared! For the test designers inconsiderately failed to choose as test categories, *i.e.* special abilities, those which the enthusiasts seem to have privately considered the only defensible and reasonable ones.

Typical of the new tests is Van Wagenen's Unit Scales of Aptitude which test "five significant mental functions" (15). One of these significant traits of the mind is the "rate of comprehension of simple paragraphs." (The size of type is not specified!) The designers of the California test of Mental Maturity say (98) that "dealing only with mental ages and intelligence quotients obscures and ignores the separate important factors." They wish (very laudably, one feels, in view of the state of affairs in current mental testing) to "reduce the 'mystery' which has surrounded the meaning of mental age and intelligence quotient." So they offer a profile of separate subtest scores which "analyzes and summarizes the various factors which are measured by the test situations." The ACE examination recommends separate 'linguistic' and 'quantitative' scores (L and Q); but analysis with college students shows (97) that the Q score does not predict achievement in mathematics any better than the L score and is less accurate in this respect than the total score.

Concerning this procedure Kuhlmann comments (15, p. 209): "We do not believe there is much merit in labelling tests as regards functions measured . . . first, because it cannot be done correctly by inspection, and secondly, because these labels are not of much value until we know also how these functions enter into school achievement." The objections could scarcely be more reasonably and successfully stated. Wells, however, seems content to accept these self-styled categories of test designers. He speaks of "disparate functions as, e.g., vocabulary range, abstracting power and imaginative richness" (15), and approves of the use of a few well-defined subtests as in the Wechsler-Bellevue or Detroit Tests of Learning Aptitude because "one acquires a feel for a wide range of responsive adequacies both quantitative and qualitative that is not approached with numerous discrete items of the Binet type" (15, p. 254). Similarly, Garrett (15, p. 257) comments on Thurstone's "Tests of Primary Abilities": "From both the theoretical and practical points of view such an approach is vastly superior to that taken by makers of omnibus tests who hope by averaging scores on a hodge-podge of functions to obtain, finally, a measure of some worthwhile ability." This observation, however, must not be classed with the other slumping theories above, since, as will become evident in the following discussion Thurstone's categorizing stands on a totally different footing.

In short, a survey of current literature reveals every possible

variety of divergence as to the objectives of intelligence testing. Intelligence is abstract thinking; it is concrete thinking; it is verbal skill; it is manipulative ability; it is innate; it is a set of acquired skills; it occurs equally in all activities; it cannot be measured by sampling; it is one thing; it is a host of things; it is a few distinct, clear-cut aptitudes. The "wave of the future" in intelligence testing is suspiciously like that in international politics. The theorists change their viewpoints with facile irresponsibility, bowing to the expediences of clinical or guidance fashion rather than to the impact of any new data or reasoning. When the reviewer comes to feel that confusion has surely made his masterpiece, he will find surprises still in store as, for example, when he encounters two or three quite incompatible viewpoints simultaneously held. One of the most popular of recent college textbooks for guidance workers and teachers (44, p. 260) tells its readers that "*intelligence is the capacity for learning, plus the useful information, skills and attitudes which the individual has gained from reacting to his environment.*" A page later, however, it has become just "*the power to learn.*" Of the first definition the writers say: "This rather liberal conception of intelligence permits it to fit into the educational program and also places in an acceptable light the majority of devices for the measurement of general mental ability." In short, a pleasant Utopia is guaranteed. Henceforth every test designer is guaranteed an "acceptable light" for his products, and every teacher is assured of an intelligence measurement adapted to the preconceptions of his local school system! The designing of intelligence tests, if not the passing of them, has been made foolproof.

#### THE MEANS OF DEFINING INTELLIGENCE

The regression that has taken place in precision of thinking concerning the measurement of abilities has naturally led to increased confusion in all associated problems. Whereas most results indicate a cessation of intelligence growth at 14 years, Thorndike (111) and Teagarden (101) discover that the curve does not flatten out until the late teens or early twenties. The problem is naturally insoluble so long as different experimenters employ different concepts of intelligence. Again, the whole development of a technical "scholastic engineering," whereby success in various school subjects can be analyzed in known vectors and predicted with known standard error, becomes vitiated by such fluidity of concepts. Finally, by such definitions of intelligence as that of Greene

and Jorgensen, the nature-nurture problem itself vanishes as does, for that matter, most psychological research directed to clarifying the field of abilities.

We are reluctantly compelled, then, to abandon the search for precise and tenable theories among the material of recent discussion and to turn with behavioristic sophistication to seeking the present definitions of intelligence through finding out what mental testers actually do rather than by asking what they think they do. This also has the advantage of enabling the survey to include the work of those test designers whose consciousness is unclouded by any such verbal superfluity as a directing theory.

A review of the literature associated with the 44 tests listed on p. 153 shows that the following methods of validation are about equally distributed among them:

- (1) correlation with other intelligence tests, or subjective ratings of "intelligence";
- (2) inspection for increase of score with age;
- (3) estimation of efficiency of prediction of (a) scholastic or (b) social and occupation achievement;
- (4) examination of amount of separation achieved between mental defectives or superior deviates and normals.

Less common and less uniformly applied methods are:

- (5) examination of normality of score distribution;
- (6) examination of internal consistency as evidenced by (a) subtest intercorrelations or (b) item analysis.

Many tests employ two or more of these criteria, without discussing, or examining experimentally, their compatibility. The Chicago nonverbal test, for example, uses correlation with chronological age, normality of distribution, correlation with other tests, and comparisons of normal and feeble-minded. The Henmon-Nelson test (51) validates against scholastic achievement, against other common intelligence tests, and through the comparison of individuals of "known superior and known inferior ability."

That the reactions of psychologists against some of these methods is not more unanimous may perhaps be most kindly ascribed to the dulling effect of custom. Spearman, it is true, entered a spirited protest (14) against basing the validation of the Stanford revisions of the Binet on the tendency of the score to increase with age and allowed himself to wonder why the mental testers did not simply judge the child's intelligence by counting his teeth. Among the tests which depend largely on age validation one

must mention the Kuhlmann-Anderson, the designers of which define the discriminative capacity of a subtest by the amount and steadiness of increase of score with age, and selected 35 out of 100 possible subtests because they showed the greatest and most consistent age increment. Garrett's comment seems adequate refutation of this practice: "Since physical strength, motor agility, and emotional control (to mention a few) all increase with age, mere increase in score with age is no guarantee that a test is an adequate measure of intellect." *jk*

Equally fraught with error is the method of comparing the truth of newer tests against the statements of older tests, or older preconceptions as embodied in estimates. Such deference to holy apostolic succession surely indicates the viewpoint of a theologian or a lawyer and has no place in science. Among physical scientists a physicist who checked the accuracy of a modern chronoscope against the fourteenth-century clock of Exeter Cathedral would rightly be considered a little too eccentric. The mass eccentricity of psychologists is hidden only from themselves. True, the comparison of a test with several tests removes statistically certain kinds of errors. It does not remove the error of whatever misconception is common to a whole generation of psychologists. ✓

Almost as unsatisfactory is the comparison of tests on the basis of their discrimination of superior deviates, normals, and mental defectives for, as Doll is constantly reminding academic psychometrists (32, 33), mental defectives do not differ from normals only in intelligence and "we in the field of mental deficiency greatly deplore the current tendency to segregate inferior deviates by means of intelligence tests alone." As for superior deviates, they may be anything from dictators or communist intellectuals to members of the Junior League or chess virtuosi—according to our social viewpoint. Quite apart from the mixed grounds on which individuals are judged as geniuses or defectives, we have to reckon with the well-known sociobiological fact of genetic adhesion of distinct psychological traits, as a result of class stratification and assortative mating. If epilepsy becomes linked with low intelligence because mental defectives marry epileptics, with greater than average frequency, are we to include epileptic symptoms in an intelligence measurement?

Among these shallow "rules of thumb" for constructing intelligence tests one can perhaps respect most the uncompromising statement of a certain class of applied psychologist that he is not



at all interested in intelligence but only concerned to establish a correlation between a test and certain kinds of life success (Criterion 3, above). This view is also given intellectual respectability by Thomson, who asks why psychometrists want to put such intermediaries as "intelligence," "mechanical aptitude," or "factor w" between their particular concrete tests and a particular concrete life situation (124). The alternative is to record that a particular test, say No. 5796, correlates  $0.82 \pm 0.03$  with success in, say, the High School English course in Middletown, or that No. 64d yields a prediction, defined by a standard error of estimate of 40%, of vocational success as a street car driver under the street conditions of 1939.

Whatever may be said from a practical standpoint, about the shortsightedness of these techniques of avoiding intermediary reference values, the outstanding objection remains that they offend scientific curiosity itself. It is a sad day if psychology is in danger of forgetting that it exists for more than guidance or education. Moreover, countless important investigations have been made with the very definite common-sense orientation that scholastic or occupational success is not synonymous with intelligence. Success in any field is surely reasonably to be considered a result of intelligence, plus more specialized abilities, plus temperament and character traits.

Line and Glen (65), for example, investigated the relationship between classroom morale, intelligence, and achievement, and concluded: "The underlying cause of the varying degree of correlation between 'g' and school achievement appears to pertain to the efficiency of the motivation." Such an assumption—that intelligence is never the sole factor in any real life situation (as distinct from a controlled experimental situation)—is found in the researches also of Burt (17, 18), Gates (41), Proctor (81, 82), Miller (79), Madsen (69), and Shewman (87), to name but a few.

An even more fundamental objection to dropping reference concepts lying between the test and the situation is that life, as Heraclitus so long ago reminded us, is a constant flux. Success in English in School A will not correlate with a given test to the same degree as in School B. A test of engineering ability in 1920 may not prove a test of engineering ability as engineering is understood in 1950. A measure of social intelligence in Russia may predict only a proneness to social ostracism in New England (*vide* Gorki). Tests which need to be scrapped with every slight spatial or tem-



poral change are not very economical. On the other hand, if it is only necessary to re-evaluate from time to time the "real life" success situations in terms of a few unchanging reference factors, *e.g.* intelligence, mechanical aptitude, verbal ability, for which there are standard tests of known validity, psychometrics has some claim to be a science.

Lastly, we come to the validation of tests by internal consistency. This, unfortunately, means many things to many people. Some hold no brief for demanding high intercorrelations of subtests yet gladly proceed to "item analysis," rejecting items failing to discriminate in line with the whole test, which theoretically amounts essentially to the same process. Others, strangely enough, demand low correlations between subtests. Thus, Greene (15, p. 321) apparently approves of adult tests having subtests "selected to have high correlations with a criterion and low correlations with each other, as in the case of the United States Army Tests." Whatever else we may feel about this criterion, which is widely entertained, though less explicitly, by several clinical psychometrists (see, *e.g.* 97), we have to add that its complete consummation is a logical and mathematical impossibility.

The question of internal consistency will be taken up afresh below. The remaining criteria can be briefly dismissed. Normality of distribution is found with thousands of measurements having nothing to do with intelligence and is sometimes absent from extensive intelligence surveys, as a result of systematic social and biological transformations. Finally, selection of subtests by "common sense," which has entered into test construction principles far more than designers are ready to admit, and which has saved them from the most hideous consequences of their own theories, has three drawbacks. The first lies in the very fact that it has saved psychologists from truly meeting their hypotheses face to face. Secondly, it must condemn them to forfeit any professional right to design tests, since the general public has quite as much common sense as psychologists; and, thirdly, the specific recommendations of 'common' sense do not, in fact, ever appear common to more than one person.

All in all, the validation procedures cannot be said to hide any richer or more precise concepts about intelligence than do the consciously elaborated theories, and frequently they are less intelligent than their rationalizations.

When one considers that a way out of this bankruptcy was

indicated at least a generation ago, first implicitly by Galton and then by Spearman (93), Garnett (38), Thurstone (118), Burt (16), Hotelling (58), Holzinger (57), Kelley (61), Thomson (108), Wilson (132), and other members of the galaxy of brilliant mathematical psychologists who have developed factor analysis, one is amazed that any competent psychologist is content to continue discussing and "investigating" tests in the limited language of clinical impression or within the shackling superstitions of educational tradition. Factor analysis does not bring in sight the end of all disputation, but it at least transports investigation to an objectivity far above that amateurish level of evaluating abilities which the psychologist has all too long been content to share with the layman.

The reluctance of the majority of American psychometrists to erect mental test practice firmly on a vector basis seemingly springs from very diverse and partly unconscious roots. First, there is a tendency to identify factor analysis with that armchair faculty psychology to which this generation of psychologists has been so strongly negatively conditioned. Secondly, there is the ancient suspicion of many clinical psychologists that error consists of lies, damned lies, and statistics. Thirdly, there is the fact that factor analysis is alien in origin, at least to the slight extent that English psychology is alien. Fourthly, there is a widespread impression that factor analysts disagree chaotically among themselves.

Only the last of these requires discussion. A persistent cause of misunderstanding is the continued statement by some psychologists of the Spearman two-factor theory in the form it reached a decade ago rather than in the more developed form admitting group factors (*i.e.* a three-factor theory) as it appears in the later work of Spearman (94) and Holzinger (57). Setting aside whatever is peculiar to the approaches of Hotelling and Kelley, which have not the same objectives as those analyses with which we are now concerned, we are left essentially with the varying systems of Spearman, Thurstone, and Thomson. The divergence is greatest between Spearman and Thurstone, on the one hand, and Thomson, on the other. But Thomson's disagreement with Spearman is one of interpretation rather than of statistical fact. As Guilford (45, p. 466) points out: "Thomson does not deny the possible existence of something that can be called 'general ability'." The decision between a general factor and a sampling theory cannot be made on

statistical grounds alone. But Spearman has given cogent general and empirical reasons for considering the general factor a more probable interpretation (95). Additionally, Thouless (115) has pointed out that the general factor theory meets the statistical facts with fewer specialized assumptions, whereas with the sampling theory a hierarchy is only one of several possible sequels. However, this disagreement is somewhat irrelevant for psychometric practice, since both theories can lead to the same procedure: the selection of tests which correlate highly with as many other tests as possible in the battery.<sup>1</sup>

Realization of the common destiny of the Spearman and Thurstone approaches has perhaps been obscured by a certain intransigence in both parties to the discussion. Thurstone's whole theory is, of course, essentially a most brilliant generalization of the special and restricted principles in Spearman's own approach (a generalization embryonic in Garnett (39)), arising from his perception of the wider implications of that penetrating pioneer work. It is probably unfortunate that when Thurstone entered upon this flight of creative thought he deliberately made difficult any return to the firm ground of Spearman's more pedestrian progress. For he introduced conditions which precluded his discovering in any set of correlations the simple general factor which Spearman had shown to be so widespread in its occurrence.

When Thurstone's epoch-making study of Primary Mental Abilities appeared (119), innocent of any general factor, Spearman (95) showed, by reworking the data which Thurstone so thoroughly sets out, that the experimental results were quite compatible with the emergence of a considerable general factor. A similar complete demonstration of the general factor resident in Thurstone's correlations was made by Eysenck (34). Guilford has shown that with a slight modification Thurstone's conditions will not preclude the discovery of a general factor. Thurstone's most recent study (121) splendidly confirms the seven or more group factors, on oblique axes, of his first study, but shows at the same time that these factors themselves can be analyzed into what he calls a "second order general factor," which he claims is uniquely determined. The convergence of Spearman and Thurstone is now

<sup>1</sup> Sampling would seem actually to indicate either of two procedures: (1) taking as many tests as possible that do not overlap at all, in which way one might miss large areas; or (2) taking tests which overlap greatly because they sample the greater part of the available sampling area.

complete, barring certain diplomatic formalities. Spearman finds certain group factors, and Thurstone has a general factor. But Spearman introduces his group factors to the reader with a cold and perfunctory politeness, while Thurstone's general factor is only permitted to enter society as a "second-order factor" after the "primary abilities" have made off with all of the actual test variance.

A masterly survey<sup>2</sup> of the whole psychological and philosophical status of factorial concepts has recently been made by Burt (19) in which he shows, as Holzinger (56) has done, independently, that the results of any one factor analysis may readily be transformed into any other. Far from being mutually incompatible, the present factorial systems of Thurstone and Spearman are closely akin. Thurstone simply absorbs into his correlated group factors that which Spearman first extracts as a general factor. The group factors found by the two schools are, or can be, the same. Thurstone chooses to make them more salient and positive, at the expense of the general factor and the special factors unique to each test.<sup>3</sup>

Factor analysis thus does not, as had once been hoped, put mental testing on a completely objective basis, above the tumult and the shouting of the educational and clinical market place. But it does exile arbitrariness to a remote and far more restricted plane. Instead of innumerable possible assumptions about traits, we are reduced to a handful of possible assumptions about methods of analysis. Psychological considerations and such general scientific considerations as the law of parsimony have been invoked to settle on the final system of factor analysis. Is a general factor psychologically and physiologically more likely than a number of

<sup>2</sup> In parentheses one may point out that Burt's genius for hiding highly germinal contributions in obscure publications has left many psychologists unaware of that repeated anticipation of later major trends which entitles him to an almost unique breadth of original authority in factorial issues.

<sup>3</sup> This conclusion is in accordance with Eysenck's in his reworking of Thurstone's data. "An analysis by Burt's procedure appears to reconcile the two conclusions; for, with Spearman, we discover a general factor, accountable for more of the total variance than any other, and with Thurstone (34) we discover a number of group-factors having a clear psychological meaning." Eysenck's analysis shows the probable outcome of most such general and group-factor analysis. The general factor accounted for 31% of the variance, and the group-factors for from 2% to 6%, *i.e.* the general factor is about five times as significant as any other.

linked group-factors? Are the criteria of Thurstone's "simple structure" analysis in fact more simple than the assumption that the mind operates as a whole? In the end we may have to admit mental traits by *fiat*. But the fiat can be decided by a ballot of psychologists on a simple issue of two alternatives—general factor or simple structure. For his part, the writer has no hesitation in publicly casting a vote for a general factor, since it retains the utility of group factors, while fitting in with the administrative convenience of an IQ and, what is far more important, with what is generally known about the mind.

The suggestion that mental testing be shifted wholly on to a factor basis cannot be left without reference to the practice of terming vectors "abilities." Whereas many psychometrists seem to be afraid that factor analysis drives abilities into becoming ghostly, mathematical intangibles, the mathematicians appear to be equally worried lest mathematical factors become too reified. Thomson, in particular, has protested that factors are not "entities" or "organs" and have no "real existence." Others have echoed this necessary warning but have gone further and denied them even the status of causes. Spearman has issued the modest and timely reminder that a factor is "one of the circumstances, facts or influences which produce a result," a statement beyond cavil.

Obviously "g" is no more resident in the individual than the horsepower of a car is resident in the engine. It is a concept derived from the relations between the individual and his environment. But what trait that we normally project into and assign to the individual is not? The important further condition is that the factor is not determinable by the individual and his environment but only in relation to a group and its environment. A test factor loading or an individual's factor endowment has meaning only in relation to a population and an environment. But it is difficult to see why there should be any objection to the concept of intelligence being given so abstract a habitation when economists, for example, are quite prepared to assign to such a simple, concrete notion as "price" an equally relational existence.

A more disturbing difficulty resides in the possibility that group-factors (or "primary abilities"), in contrast to the relatively biologically founded general factor, may prove to be highly dependent, for their existence and form, upon the cultural pattern. For it seems quite likely that a few of them will prove to have no



more permanent existence than our current classification of school subjects or our present-day syndromes of occupational skills. Thurstone's practice of rotating axes to give them "psychological meaning" to some extent binds him to chasing his own test vectors and to searching for clusters<sup>4</sup> created by his own selection of tests which are sometimes so similar as to cover only what in Spearman's way of speaking would be so narrow as to be a "specific."

x All too often when the scientist peers into the hardest and most opaque surfaces of nature he sees only the reflection of his own face. Our vectors turn out to be the foci of our own interests. Whether psychometric considerations direct us to analyzing into a general factor and relatively shallow group-factors or only into large, but related, group-factors alone, it seems that we may have to decide by fiat the precise directions, by means of reference tests and populations, of these group-factors or primary abilities.

We have to conclude this phase of the survey, therefore, with the somewhat unwelcome practical suggestion that factor systems now require an act of psychological decision. The suggestion calls for the setting up of a psychometric definitions committee to inquire as to which system offers the greatest convenience to the greatest number. Its task would be to fix vectors accordingly, by means of specific reference tests and defined populations. Such an intrusion of mere arbitrament and ballot into scientific affairs may seem a confession of failure, yet in fact it is as necessary and natural at the point we have now reached as is the creation by fiat in the physical sciences of units of measurement. Only by some such step can research and practice in psychometrics be rescued from its present futility and erected on a basis permitting precise, comparable, and permanent findings. A further refinement of this theoretical position is developed in the next section.

#### THE CONSTRUCTION OF ADULT INTELLIGENCE TESTS

If the construction of adult intelligence tests were considered satisfactory only when explicitly founded on a satisfactory theory, the majority of existing adult tests would have to be abandoned. The performance tests would be rejected *in toto* because of insufficient saturation with the general factor found in intelligence tests at large. The verbal tests would be convicted of heavy weight-

<sup>4</sup> Not directly, by seeking the center of the cluster, but indirectly by finding the bounding hyperplane with which most test vectors have no alignment, *i.e.* by avoiding the cluster.



ing with a verbal group-factor. Some would have overlapping subtests. Many would be found unnecessarily contaminated with knowledge, special information, or skills. Let us, however, for the moment accept this crippling debility in basic theoretical constitution and proceed rather to examine less central constructional features of the existing tests.

A survey of the *Mental measurement yearbooks* (14, 15) shows that the most frequent comments and criticisms on the construction of adult tests concern: the length of the tests (too short or too long), the absence of language or of number subtests when the tests are intended to select candidates for linguistic or mathematical education, the insufficient provision of variety among subtests, the use of point scales or of age scales according to the predilections of the critic, the weightings of subtests or points, the subjectivity of scoring, the defectiveness of reliability or consistency measurements, the erroneous arrangement of order of difficulty of items, the use of an unsuitable vocabulary, and the setting of ambiguous or defective instructions. Let us deal with these in order.

A glance at the length of the principal tests yields the following results, in which the first figure indicates the shortest possible time for the test and the second the length of the complete form, with all allowances for instructions and administrative requirements.

Among performance tests the Kent-Shakow takes 15-30 minutes; the Ferguson, 10-40 minutes; the Alexander scale, 40-60 minutes, the Arthur scale, 35-90 minutes. Among pencil-and-paper tests we find the ACE taking 55-60 minutes; the California Mental Maturity, 45-90 minutes; the Cattell Adult, 66-75 minutes; the Detroit, 29-40 minutes; the Henmon-Nelson, 30-35 minutes; the Kuhlmann-Anderson, 40-60 minutes; the Leiter Nonverbal, 30-90 minutes; the Ontario, 20-40 minutes; the Otis Self-administering, 35-40 minutes; the Pintner, 55-65 minutes; the CAVD, untimed, may take anything from an hour to several hours; the Revised Beta, 40-75 minutes; and the Thurstone test of Primary Abilities, 153-270 minutes. The time for such individual tests as the Binet or the Wechsler naturally depends partly on the mental tempo of the examiner and subject. The median time for the most frequently used general tests is thus about 40-65 minutes.

Judging by most comments, a test of this duration is considered by many impracticably long. In the recent military personnel testing, for example, the most widespread practice uses, at least for preliminary testing, much briefer tests while the recommendation for final testing is to use such a brief battery as the Kent Information Test, the verbal items of the Wechsler-Bellevue, and the Porteus Maze.

The problem, of course, is tied up with that of reliability or of consistency, as we shall call it, since this term is more precise and less likely to be misunderstood by laymen (and by psychologists themselves!). Is it possible to cut down a test much below one hour and still get a measure of sufficient consistency—not to mention validity—to be used as a basis for decisions affecting the individual's whole career? It may well be doubted, but the question is not at present answerable, for two reasons.

First, the consistency depends rather on the number of items answered than on the time. The number of pass or fail items seems defective in some well-known tests, sometimes because the test attempts to measure so wide a range of ability that the pass or fail range is too steep and brief for any one age sample, as may happen in the Chicago Nonverbal (age 7 to adult), the Leiter Performance (age 4 to adult), or the Binet, and sometimes because the test items are individually too "large," as in the Binet (22, 23). The spiral omnibus test is in this respect defective in comparison with a battery of well-defined subtests, for testing time is consumed in reading instructions and in constantly reorienting the subject's attitude to new items.

Secondly, the problems defy quantitative recommendations because, in spite of the great attention given in manuals to reported consistency coefficients (such that no test will be accepted without them), they are practically meaningless. By a suitable choice of population almost any desired consistency coefficient can be obtained. Increasing the age range, the intelligence range, the educational homogeneity of population and motivation will give impressively high correlations. Until these conditions are required to be standardized, so that a hall-marked consistency coefficient is available, the question of minimum consistency and length of test cannot be handled with precision. Further, it is apparently necessary to add, the test manuals need to indicate whether the correlation figure represents a split-half or a repeat consistency coefficient.

Of the problems listed at the beginning of this section the most centrally important and the only remaining one requiring more than a mere indication of the difficulties is that concerning the choice of subtests. Naturally the solution to the problem depends on the answer to the question of the nature of intelligence, as discussed in the last section, but there are unfortunately many attempts to solve it without reference to that question. Wechsler (127), after a survey of current adult tests, says: "It is surprising,

however, how many tests that form part and parcel of most scales turn out to be poor indicators of intelligence when checked against clinical experience." This is true, for example, of the Binet. Wechsler proceeds to indict "giving rhymes, checking letters, making designs, defining unusual words, repeating long series of numbers, putting together formboards."

Unfortunately one cannot report, after surveying current trends, any systematic advance in the manner of choosing subtests. Wechsler settles down to an information test, a memory span for digits, similarities, arithmetical reasoning, picture completion, block design, object assembly, and digit symbol. The information test, in spite of its seeming objectionableness, is included on the good ground that in the Army Alpha battery when "analyzed according to their correlation with various estimates of intelligence the information test, to the great surprise of many, turned out to be one of the best of the whole series." This is widely confirmed and is paralleled by Spearman's discovery that vocabulary and school information head the "g" saturation list for children in the same school system. Wechsler's inclusion of other subtests, however, such as block design and digit memory seems at odds with his own criticism above, nor can one discern any general principle in his choice of subtests.

On the basis represented by factor analysis we should get, by Thurstone's method, the subtests which Thurstone has rightly included in his primary ability battery. Similarly, on a general factor analysis, we collect a set of subtests such as will be found scattered, but not sufficiently represented, in most current test batteries.

Thorndike's CAVD, however, among a few others, stands out as being entirely composed of highly "g" saturated subtests (54). The researches of W. Brown,\* of Brown and Stephenson (13), Simpson,\* Bonser,\* Holzinger,\* Magson (70), Spearman (93, 95), Burt (16), Bernstein (9), Thorndike (114), Thomson (109), and several others working with equally high standards of population sampling and methodology agree in pointing to such tests as analogies, opposites, synonyms, deductive reasoning, sentence completion, classification, arithmetical reasoning, and comprehending directions as having repeatedly demonstrated the highest saturation with a general factor. Thomas (105), among others, has made out a case for these being the tests which require the most complex eductions of relations and correlates as distinct from mere reproduction, recognition, or perception.

\* The results of these researches are conveniently set out in Spearman (93).

In view of more recent work on the verbal factor, some of these would have to be rejected as battery mates because, despite high saturation with a general factor, they share verbal ability (119, 96). Suggestions for designing a test from constituents of high "g" saturation but without verbal content have been worked out by the present writer elsewhere (25, 27). The selection of the most valid subtests, in the light of recent work on the extent of group-factors, is an incompleting task.

But the selection of adult test subtests runs into other special difficulties. First, as Spearman pointed out in his "principle of diminishing returns" (93), the saturation of almost any performance with the general factor becomes less as higher levels of general ability are reached. Subtests of a type which correlate from .6 to .8 with the general factor among children are loaded only to .3 to .5 for adults. This may be the result partly of the individual's spreading of interest and skill with age, partly of the replacement of intelligence by habit as more activities become accustomed activities. The result might at first sight be thought to be due merely to lesser difficulty, for we know that the saturation of a test depends on its relative degree of difficulty and on the attitude adopted to it, over and above its intrinsic nature (93). But the lower saturation does not seem to be avoidable by increasing the degree of difficulty.

From a clinical viewpoint the same decay of subtest saturation has been observed by Wechsler, when, discussing adapting children's tests for adults, he says (126) that "at age seven, ability to copy a diamond is a very good indication of the child's intelligence, but if we increase the complexity of this task, say by demanding the reproduction of a bisected rhomboid, we succeed in making the test more difficult but add little to its discriminative value as a test of intelligence." The same weakening of the general factor in adult measurements is evidenced by the fact that Thurstone readily discovers the general factor for children (121) but not for adults (119). The moral for adult intelligence testing of the principle of diminishing returns seems to be that a greater number of diverse subtests needs to be used than for lower mental ages. In this respect Kuhlmann's test with 35 subtests, or the revised Binet with its even greater variety, may be as good as the CAVD with only four, despite the greater validity of each of these four.

A second difficulty specific to adult test construction is the influence of speed. The most thorough factor analysis of speed and

intelligence in children's intelligence test performances fails to reveal any separate speed factor except in very special circumstances (9, 100). Sutherland was able to find a residual speed factor after extracting "g" only when the problems were patently too easy for the children (100). On the other hand, as the discussion of the next section, on age and performance, shows, there is evidence that speed and intelligence are more separable among adults. Clinicians have also urged their distinctness, and most mental test constructors have found themselves disposed to give more generous time allowances in the adult tests or to abolish the time limit altogether, as Thorndike does in the CAVD test.

Large (66) emphatically takes the viewpoint that speed should not enter into the concept of ability and stabilizes this position by making use of the separate concepts of "power" intelligence and "speed" intelligence, implying that the former should alone be considered intelligence in adults. Kelley (15) seemingly adopts the same view, for he criticizes Thurstone's tests of primary abilities on the grounds that "speed is a function of each and every one . . . . This raises the reliability coefficients . . . but lowers the purity of the measures." It is suggested indirectly by certain researches that when subjects are allowed to work at their own tempo (*i.e.* not made to work at top speed volitionally), speed factors, notably the "f" factor of temperament (23), will be found to emerge, even with children. The will obscures natural differences of working speed.

The emergence of a speed factor with adults may thus be due to their decreasing ability or inclination to control temperamental-physiological dispositions by effort of will. Pending further research, it seems safest to regard adult tests as measuring somewhat different factor combinations under speeded and unspeeded conditions. But there is no adequate reason for valuing the unspeeded measurement as "intelligence." Indeed, the exigencies of test practice will almost certainly lead to speeded tests being given priority in personnel work if not in research.

These two sets of observation, on diminished "g" saturation of adult intellectual performances and the adult disassociation of speed from power, may be linked with an equally independent third source of evidence to create a new concept in regard to the nature of adult intelligence. The third source lies in the data on mental capacities and brain injury, principally as summarized in the contributions of Doll (31), Hebb (49, 48), Lashley (62), and Weisenburg and McBride (128). From a review of psychometric data in this field Hebb (48) has attempted a generalization similar



to that of Lashley in the field of animal studies and to the effect that a localized brain lesion produces in children a generalized impairment more noticeable than any specific functional loss, whereas in adults a corresponding injury produces more specific loss of powers and less obvious loss of "g." This he considers well substantiated, but he puts forward more tentatively a further observation that the adult deteriorations are least in vocabulary, information, and verbal comprehension and greatest in speeded tasks, abstract problem-solving, and unfamiliar performances (49, 48). The latter are the performances which in any case decline most with age and which suffer most in schizophrenia. The present writer would add that they are also the tests which show the best "g" saturation in adult populations.

The hypothesis in which the considerations of the last two sections culminate, in the opinion of the writer, may be set out most briefly as follows:

(1) Adult mental capacity is of two kinds, the chief characteristics of which may be best connoted by the use of the terms "fluid" and "crystallized."

(2) Fluid ability has the character of a purely general ability to discriminate and perceive relations between any fundamentals, new or old. It increases until adolescence and then slowly declines. It is associated with the action of the whole cortex. It is responsible for the intercorrelations, or general factor, found among children's tests and among the speeded or adaptation-requiring tests of adults.

(3) Crystallized ability consists of discriminatory habits long established in a particular field, originally through the operation of fluid ability, but not longer requiring insightful perception for their successful operation.

(4) Intelligence tests test at all ages the combined resultants of fluid and crystallized ability, but in childhood the first is predominant whereas in adult life, owing to the recession of fluid ability, the peaks of performance are determined more by the crystallized abilities.

The goodness of fit of this hypothesis to the facts of power and speed difference in adults, to the decline of speed intelligence with age, and to the physiological data for animals and men is sufficiently clear. It fits the factor analysis data equally well. For if the crystallized abilities are, as it were, a dead coral formation revealing by its outlines the limits of growth of the original living



tissue, these crystallized abilities will show approximately the same intercorrelations as the original fluid abilities.

Actually we should expect the intercorrelations to approximate the childhood values but to be systematically lower (as indeed we find in the lower "g" saturation of adult subtests) for the following reasons:

(1) Losses of particular abilities in adult life, through injury or neglect, would leave an irremediable unevenness unknown in childhood, where the omnipresence of fluid ability determines an all-round even level of performance.

(2) Since in childhood fluid ability is constantly growing, so that its discriminatory power quickly overtakes the level of any crystallized, discriminatory powers established by exercise, it is very difficult, even with extreme application of time and effort in one direction, to develop special discriminatory habits much, or for any length of time, ahead of the general fluid ability level.

(3) Moreover, the all-round inferiorities and insecurities of childhood, as well as the general, unsystematized interests and curiosities, cause a more widely distributed investment of fluid ability in building up discriminatory habits than is found with the adult whose life adjustments are better served by continuous application in one particular field of skills, *e.g.* those involved in his occupation.

In recent years several formulations approximating to the above crystallization hypothesis have been made. Lorge (66) suggested a division into power and speed. Hebb (48, 49) has independently stated very clearly what constitutes two thirds of the present theory, for he says that "intellectual power may be needed for the first appearance of the qualitatively superior response, but not necessarily for its persistence" (49), and "in any test performance there are two factors involved, whose relative importance varies with the test: one factor being the lasting changes of perceptual organization and behavior induced by the first factor during the period of growth." The present theory differs from Hebb's: (1) in identifying the first factor with "g" among children, thereby modifying Spearman's "g" hypothesis to take account of findings regarding age changes and brain lesions; (2) in considering intellectual development to be a continuous increase in the capacity to perceive hierarchically more complex relations rather than an appearance of new, qualitatively superior responses; (3) in supposing, contrary to Hebb and Lashley (62), that the high intercorrelation of tests in childhood is due to a functional unity of fluid ability and therefore, presumably, of cerebral action and is not an artifact arising from pre-established

harmony in growth; (4) in connecting with more connotations, thereby making the theory more rigid, more remote from the level of a descriptive hypothesis, more subsumptive of data from different fields.

The hypothesis also has some kinship to Spearman's doctrine of energy and engines (93). It differs (1) in incorporating data not available at the time of Spearman's formulation permitting more specific and operational definition of the "engines," and (2) in considering only part of the adult statistical general factor to be due to the operation of the "energy" or general fluid ability. The remainder of the intercorrelation is explained as a reflection of the high-water mark in a variety of abilities left by the receding tide of fluid ability, *i.e.* the intercorrelations are only historically traceable to the nonspecific fluid ability.

This *hypothesis of fluid and crystallized ability*, if sustained by further discussion, would seem to be of crucial importance to the practice of adult intelligence testing, indicating the need for a dual measure if the individual's performance in different situations and at different periods is to be predicted reliably.

The next problem specific to adult testing concerns motivation and the role of test sophistication and practice. Most psychometrists are impressed with the difficulty of getting adults to do tests at all. At the very least, their motivation lacks that first fine careless rapture with which children approach competitive tests.

Miles and Miles (76) mention "the difficulty which we and others have encountered with adult subjects in administering tests." Weisenburg, Roe, and McBride (129, p. 37) report 21% of refusals among general hospital patients invited to undertake tests, but others report up to 50%. Apparently there are no experimental findings concerning test motivation effects among adults, other than those which show that with college students, as with children, strong additional incentives produce no significant increase in score (7, 35, 71). Nevertheless, Lorge's (67) findings on adult learning suggest that some of the peculiarities of adult intelligence test performance may result from defective interest and intention and indicate the desirability of devoting time to orienting the subjects in the practice of adult testing. Teagarden (15, p. 225) recommends for this purpose such a performance test as the Kent-Shakow, which seems generally to evoke natural adult interest, for, he says, "Because of the interest, the test is valuable for establishing rapport with the adult applicant, who may be a bit fearful or scornful." Such use of a performance test before the test proper is, of course, a common clinical practice with children, but it may be quite as necessary with adults.

A related problem concerns the use of "shock absorber" or practice tests prior to the main test.

The general evidence (1, 23, 89, 110) is to the effect that practice in intelligence tests leads to appreciable gains, greatest between the first and second testings but still appreciable between the later testings. Dave (30) has shown that this practice effect is greater with respect to some tests, *e.g.* spatial classifications, than others, *e.g.* analogies, mixed sentences, and comprehension, but we do not yet know whether this difference arises from the average adults' comparative lack of previous experience in the first type of test situation. The present writer and co-workers have shown (27) that practice gives greater increments with the ACE and Binet than with a culture-free (perceptual) test or the Arthur performance scale. Vernon (123) has convincingly split up the general practice effect into practice as such and "test sophistication," *i.e.* general orientation to the examination situation and to the psychology of mental test constructors.

No studies offer direct or sufficient evidence as to the relative importance of practice and test sophistication among adult populations in contrast to children, but it seems likely that inequalities of test sophistication, if not of practice, would be a more serious problem among adults, for older adults are more remote from the examination situation, emotionally resistant to it, uninterested in competition, less likely to be acquainted with the latest fashions in intelligence tests, and unused to working to brief time limits. Consequently, with adults, the inclusion of a practice, "shock absorber" test before the true test (to be ignored in the final scoring) would seem to be desirable or even indispensable; but less than one in five of the above forty-four tests employ such a device.

#### PROBLEMS OF STANDARDIZATION AND THE METHOD OF EXPRESSING MEASUREMENTS

The new and unexpected difficulty confronting the standardization of adult intelligence tests lies in the discovery between 1928 and 1934 by Willoughby (131), Miles and Miles (76), Jones and Conrad (60), Sorenson (89, 90), and Cattell (21) that adult intelligence test scores actually decline after the age of 20 or 25 years.

Interpretations of this decline have varied, but some perspective has been arrived at through the more recent research and observation of Gilbert (42), Cowdry (29), Sorenson (91), Brady (11), Henry (52), and particularly of Lorge (66, 67), Wechsler (127), Lawton (63, 64), and Weisenburg, Roe, and McBride (129). Willoughby (131) found declines in completion, analogies, opposites, etc., but not in arithmetic, and con-

cluded that, since the last is more used in adult life, the declines in the more "literary" tests could be put down to distance from schooling and to the schooling of the older adults having in fact been poorer. There is, of course, ample evidence of negative correlation between adult intelligence and school-leaving grade (52, 82). Cattell (21), who found decline greater in the less skilled occupational groups, concluded the effect to be partly biological and partly due to "promotional impoverishment" of lower occupational levels. Miles (75) supplied the necessary proof, lacking in other studies, that the decline occurs not only as between serial age samples of the general population but in any one set of individuals tested after the lapse of years. Such results indicate that much the greater part of the decline must be assigned to an inherent process and not to educational selection, promotional impoverishment, or any other social mechanism.

The magnitude of the decline between 25 and 65 in the early studies was considerable, often in the neighborhood of one standard deviation. In the very carefully sampled study of Jones and Conrad (60) it was found that the more representative the sampling, the greater the decline, since the more deteriorated of the elderly people tended to avoid testing. Miles and Miles (76) found an age-intelligence correlation of  $-.50$  over the range 20-95 years, but a lower correlation over shorter periods. They also demonstrated that the decline was smaller for the higher educational groups. The age from which decline begins was found to be, on the Army Alpha, between 19 and 21 (60). Miles and Miles (76), on the Otis, found the peak about 25, while Wechsler (127), with the Wechsler-Bellevue, found it at about 23. In all these inquiries the change is very slight from the first five years after the peak. The curve then settles down to a straight line from about 27 to 65, after which it falls more steeply.

Much remains to be discovered concerning the forms of mental ability which persist or decline together with age, for knowledge here would throw light on the nature of intelligence and on the necessary observances in adult mental test construction.

The work of Willoughby, and Jones and Conrad, showed, on the whole, that the most rapid decline took place in those tests generally considered among the best for measuring intelligence, *e.g.* analogies, common sense, and number series completion. General information, synonyms, opposites, and arithmetical reasoning are relatively well maintained. The fact that the age peak of performance for verbal abilities appears later than for nonverbal powers led Weisenburg, Roe, and McBride (129) to consider that the important distinction in age decline might be between verbal and nonverbal material, but the conditions fixing the peak of the adolescent upgrade and those governing the steepness of later decline may, of course, be quite different, in one case the

emphasis being on learning and in the other on forgetting. In their own study they found very little decline on vocabulary, reading, spelling, and arithmetic tests (129). This selection, as they point out, is more consistent with the hypothesis that continued use and experience suffice to determine lack of decline, though they wisely add that one must consider also "the possibility that (these tests) involve abilities which mature late or decline slowly."

Speed has so far proved the most clear-cut factor distinguishing between declining and nondeclining tests, as indicated in our general discussion on intelligence theory above.

Miles (76) showed by a comparison of the Otis Speeded and Unspeeded that speed declined faster than power, at least until late maturity, when the decline in power became equally apparent. It is well known that tests of sheer reaction time, sensory acuity, motor skills, and perception come to maturity earlier than reasoning powers and decline sooner, so that the partialling out of such elements from intelligence tests might conceivably leave them practically immune to age effects. Lorge's (66) inquiry offers the most complete data on the question. He gave 11 "intelligence" tests to 145 adults ranging from 20 to over 70 years of age. The scores with age correlations were all negative, ranging from  $-.275$  with the CAVD (unspeeded) to  $-.485$  with the Otis A (speeded). Lorge concludes that the "reported deterioration is more apparent than genuine" when power tests rather than speed tests are used. Correcting for speed gives a plateau or even a slight age rise in power performances, though he admits this result may be an artifact due to the death rate being higher among the less intelligent. His later work on age and learning (67), revealing the difficulty of getting adequate motivation in adults, raises the further possibility that part of the age decline of intelligence score in adults is due to inadequate drive, but, apart from such alternative suppositions, the conclusion that the decline of power with age can be considered negligible is distinctly questionable.

The issue of speed and power separation cannot be considered solved. As mentioned above, a number of researches have shown (9, 73) that it is not possible with children to extract a factor of speed as distinct from the general factor of power in intelligence tests. Even in Lorge's research, the CAVD with unlimited time correlated  $.85$  with the speeded Otis. It is possible, however, that with adults factor analysis will give different results, justifying a conception of intelligence into which speed does not enter. Even so, it seems essential at present that adult tests, preferably with liberal time allowances, should be standardized anew for each decade. A mere age allowance, based on change of mean, is not sufficient until we know how the standard deviation varies from decade to decade. However, the Wechsler-Bellevue standardiza-



tion, which admits a 20% decline in norms between 20 and 60 years, is probably as near as we shall come for some time to a test satisfactory with respect to age change, though the research work with the Otis and Army Beta has provided sufficient data for working out an allowance on these tests also.

A more intractable problem in scoring and standardizing adult intelligence tests concerns the use of the intelligence quotient. All the accumulating criticisms of the IQ in work with children receive such added power from the special difficulties of the adult situation that a considerable proportion of psychometrists does not hesitate to condemn its use in this field. Greene justifiably observes (43): "Standard scores seem to be already preferred to IQ's when dealing with adults, as shown by the frequency of their use among college and employment bureau records." Yet it is easy to see that much of this condemnation is premature, a result of slavery to fashion and the obsession towards change for its own sake. When the factorial content of intelligence is settled, there should be no difficulty in discovering, with far more accuracy than we can now aspire to, the true age of cessation of intelligence growth and the consequent denominator to be used in working out adult IQ's. Adult IQ's below 100 can then be calculated in the usual way, and those above 100, by assuming that IQ's above 100 are distributed on the upper half of a normal distribution curve, or as a reflection of the distribution of the lower half or on a curve which is a replica of whatever distribution curve has been found for children. This standardization procedure has had its practicability demonstrated for at least one adult test since used for a sufficient variety of purposes in surveys, guidance, and research (21). To express adult intelligence in terms of IQ is admittedly artificial—as artificial as equating the power of a airplane to that of a number of horses, or expressing the brilliance of an electric light in terms of candles, but the critics' objections on this score seem somewhat ill-considered.

As alternatives to the IQ one may use a percentile score, a standard score (including such derivatives as the T-score), or one of the growth constants that some researches have suggested. Since the translation of an IQ into a percentile score or a standard score requires but a few seconds, the IQ can be considered to offer what a standard score offers plus the further significance which attaches to the ratio itself. For those who are accustomed to use intelligence tests over the ranges of childhood and youth, the IQ carries an immediate meaning in terms of relative ability, of level

of brightness, and of certain scholastic clinical and occupational reference points. It is hard to see what is gained by substituting a stark standard score for the richer connotation of the universal IQ.

If any real objection exists to the IQ, it exists among the systematic difficulties already existing in the device as a measure even of children's intelligence rather than in those peculiar to adult testing. Let us view these difficulties in the light of alternative measuring devices suggested.

Suggested substitutes for the IQ are almost as numerous as mental test designers. The last 20 years have seen the Index of Brightness by Otis, the Intelligence Ratio of the Myers Mental Scale, the Coefficient of Intelligence of Yerkes and Wood, the Absolute Mental Units of Arthur and Woodrow (4), the absolute scaling of Thurstone (116) and of Richardson and Stokes (83), the equal units of Thorndike (114), the Heinis Personal Constant (50), and its modification in Kuhlmann's "P.A.," or per cent of average, and so on.

Most of these modifications, of course, imply the abandonment of the idea of mental age as well as IQ. The substitution of a point scale for mental age scoring has been a growing demand among applied psychologists. A. W. Brown (15, p. 223), for instance, approves of the Herring-Binet Revision because "first, the Herring is a point scale which, according to most test theorists, is an advantage." But the substitution of a point scale does not mean for most psychologists any tampering with the notion of mental age as such. Their consent extends only to the method of arriving at mental age—namely, through a point scale.

Important theoretical possibilities exist in these alternative conceptions, and undoubtedly it is time for full justice to be done to them, but the present survey cannot undertake so ambitious a digression. Some of them, like the IR, combine units of age and raw score in a grotesque mongrel measurement. Others build a delicate superstructure of exact calculation on the perilous assumption that one can arbitrarily fix "equal" unit intervals of mental growth in terms of raw scores.

Richardson and Stokes (83) confirmed Thurstone and Ackerson's finding (120) that the spread of intelligence score at any age is proportional to its mean level at that age and that sigma divided by the mean gives a constant of about 0.18. Studies of growth on absolute scaling generally give a G curve.

<sup>1</sup> See, for example, the data of Bellis, C. J. Reaction time and chronological age, quoted by Lorge, I. (66, p. 109).

Heinis' notion of a growth factor (50), as expressed in the formula  $Y = B(1 - e^{-x/d})$ , certainly merits further investigation; for there is no particular reason for supposing any solemn promise on the part of nature that so simple a ratio as the IQ shall be exactly constant or the one infallible index for defining the growth curve. Yet, as it is worked out at present, there is much in this growth constant that is arbitrary and unsatisfactory. For instance, the particular curve which Heinis settles upon supposes "that the growth of human intelligence does not normally stop from the time of birth to old age. This progress, while remaining positive, diminishes from year to year, so that by the age of 40 the normal man has practically attained his maturity" (50, p. 167). Garrett, commenting on the Kuhlmann-Anderson use of this constant, says: "Heinis units are derived values obtained from fitting an arbitrarily selected exponential equation to certain poorly prescribed data, gathered by Heinis from various sources" (15, p. 227).

Woodrow (134) has recently suggested a formula which yields a curve excellently fitting the observed development of intellectual abilities as well as many other psychological functions. But there is no suggestion that any particular constant or constants therein represents the individual's endowment.

The sole basis for real dissatisfaction with the IQ resides in some researches claiming to show a systematic inconstancy (84) in the direction of a decrease of low and an increase of high IQ's with time. The Heinis constant specifically sets out to avoid this.

An investigation of this issue by Psyche Cattell (20) reveals that, though below average IQ's undoubtedly fall, those above average remain constant. For the latter values the Heinis constant is less constant than the IQ. The author points out that the fall of IQ in defectives may be an artifact arising from their limited environments in relation to the test requirements and adds, regarding the previous findings of IQ inconsistency, that there has been unwarranted generalization, since the observations originate from only one test—the Binet. Errors of sampling in the upper age level standardizations, contamination of tests with scholastic skills, scholastic selection, simple regression to the mean, etc. obviously need to be investigated before assuming inconstancy of the IQ. Such considerations have been emphasized in a recent thorough review of IQ constancy in this journal by Robert Thorndike (113).

Before abandoning the IQ so precipitately, its critics might, moreover, first investigate possible modifications in its calculation. For example, should standardization be through regression of score on age or age on score? Should the zero of innate development be placed at birth or should nine months be added both to the age denominator and to mental age performances? The latter would seem to be more reasonable if intelligence is more dependent on

inherent maturation than external stimulation. Should the IQ be based on a measure of the pure vector rather than on a sum of subtests containing an unknown bonus of special abilities? And so on. Apropos of the possible significance of contaminating a measurement with unintended specifics, Thomson comments on Thurstone's Test of Primary Mental Abilities (15, p. 260): "My first reaction, however, was one of great disappointment that Thurstone, after all the spade work he has done towards measuring these primary abilities as accurately as possible, should have descended to the crude plan of offering the sum of the raw scores in a couple of tests as the measure of each factor." But this is done every day by those who impute the inconsistency of the resulting IQ to the psychological or biological spuriousness of the IQ itself rather than to the variable specifics and errors they have included in their contaminated measurement. Until growth curves for general abilities and primary abilities are better understood, no advance in precision on the IQ seems to be obtainable by shifting to other "constants."

The notion that the IQ contains everything in a standard score and more besides is potentially true, but is it not entirely a valid and practical argument at the present moment, for the standard deviation of the IQ has in fact not yet been fixed to the satisfaction of all psychologists.

The early Binet testing seemed to approach an S.D. of about 11 points of IQ (16). Terman's first extensive California studies (102) indicated that it lies near 13 points. More recent work with children (103) and, less extensively, with adults suggests its stabilization at 15 or 16 (104). The ideally complete Scottish survey (136), however, showed a standard deviation of 16 to 17 points, and some Binet surveys have given 19 points. On turning to group tests, larger standard deviations are obtained. The present writer, finding S.D.'s of 25 and 27 (21, 24), has argued (23) that since the scatter is greater with the more recent tests, freer from scholastic influence (which forces the dull and slows the bright), the true S.D. of IQ's is probably significantly higher than that which we now accept. Thomson concedes that this view is possibly right (15, p. 210) and adds that the experience of Moray House in adult testing with newer group tests indicates that the scatter of the obtained IQ's needs to be reduced by about a third in order to produce values comparable with the measures of the Binet test, which has a diminutive scatter. Some recent unpublished findings by Stephenson at Oxford, working with perceptual tests free from verbal factor loadings, concur in indicating a far greater adult variability than had previously been supposed and would support the above argument that the scatter on less educationally biased general ability tests will be found larger than that now accepted as normal.

These uncertainties regarding the rate of exchange between IQ and standard score are admittedly an argument against the present use of the IQ for adults. A few years of research, however, should suffice to establish the relation. To go off the IQ standard, therefore, at the present juncture, for this reason, would seem quixotic as far as theory is concerned and wasteful with respect to the accumulation of useful knowledge.

From a survey of these and other practical and administrative testing problems one is forced again to the conclusion set out in the early part of this article: that the most intensive research on applied psychometrics or on restricted theoretical issues is vain so long as the central question of the nature of intelligence and human abilities is neglected and unsolved, and that adult intelligence testing in particular requires for its successful prosecution the solution of this problem.

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# THE STANDARDIZATION OF THE TERMAN- MERRILL REVISION OF THE STANFORD-BINET SCALE

## A SPECIAL REVIEW

BY HENRY E. GARRETT

*Columbia University*

McNEMAR, QUINN. The revision of the Stanford-Binet scale: an analysis of the standardization data. Boston: Houghton Mifflin, 1942. Pp. 185.

This is a companion and supplementary volume to Terman and Merrill's *Measuring Intelligence*. Essentially, it is a presentation, summary, and analysis of the data upon the basis of which the 1937 revision of the Stanford-Binet scale was constructed and standardized. Except for Chapter I, the book was written entirely by McNemar, who is certainly to be commended for the compact and succinct form in which the material is presented. A minimum of long-winded discussion and speculation makes the book much easier to read than one might have anticipated.

The first chapter (contributed by Terman) summarizes briefly the new revision procedures, describes the subjects, methods of test placement, characteristics of age-scales in general, and other pertinent facts about the test. Chapter II shows the distribution of IQs from Forms L and M to be essentially normal over the different age groupings. Mean IQs run 3-6 points above 100. This is as it should be, according to McNemar, since the standardization group did not adequately represent the lower occupational levels or rural groups. When corrected for these factors, the mean IQ by age levels will be close to 100. The SDs for Forms L and M are close to 17 IQ-points. This means that the spread in the New Revision is considerably greater than in the Old (in which the SD was approximately 12 IQ-points), and hence IQs from the two scales are not directly comparable.

Chapter III presents an analysis of IQ distributions by age and by grade. Several interesting facts appear in the tables which give the variabilities of MA and IQ by age and by grade. The SDs of the IQs remain approximately equal at successive age-levels,

while the SDs of the MAs show a fair—but not entirely regular—increase with age. These two necessary conditions for IQ constancy are, therefore, reasonably well fulfilled. Age and grade groups are about equally variable in IQ. But the range of MAs within any given grade is astonishingly large. One finds the MA range in the 6th grade, for instance, to be more than 6 years, and the range is even greater in grades 7–12.

In Chapter IV the main sample is broken down into occupational levels and into urban and rural groupings; and the mean IQ and variability of each group are given. Children whose fathers are in the professional group average 18–20 points higher in IQ than children whose fathers are in groups IV, V, and VI (rural owners, semi-skilled, slightly skilled) and this difference holds at all age levels. McNemar very wisely refuses to commit himself either to an all-out genetic or environmental explanation of these considerable differences. But at least one author (Neff, W. S., Socio-economic status and intelligence: a critical survey, *Psychol. Bull.* 1938, 35, 727–757) has been willing to interpret IQ differences between occupational levels as due mainly, if not entirely, to environment. To the reviewer the fact that (1) the mean superiority in IQ in the higher occupational group holds at *all* age levels, and (2) the further fact that IQ variability is as great at the lowest as at the highest occupational levels (suggesting that environment did not exert a *generally* raising or lowering effect) argues as well for a basic genetic determination. Further argument on the same point might run something like this. Let 20 points of IQ represent the maximum effect of environment (mean difference between occupational levels I and VII). Now the maximum effect of environment plus all other factors can hardly exceed 90–100 IQ points, since this is the total range at a given age. If 20% of this range is assigned to environment, roughly 80% of the variability in IQ among children of the same age may be ascribed to biological factors plus other non-environmental effects (e.g., chance errors and the like). This conclusion is speculative, to be sure, but it is in rather close agreement with other analyses (Burks, Woodworth).

Chapter V finds little evidence of sex differences either in MA or in the separate items of the test. In a very general way, girls tend to surpass boys at the lower age levels, and are better on certain manipulative tests (buttoning, knot tying); boys surpass girls at the upper age levels and on tests of space perception,

arithmetic, and puzzle-like tests. But the tests showing sex differences are few relative to the scale as a whole.

Data on the reliability (Chapter VI) of the New Revision are much more completely presented than was true of the Old Revision. Instead of a single standard error of an obtained IQ, McNemar has by the use of two methods (which check closely) determined standard errors for different IQ levels. The s.e. for IQs of 130+, for instance, of 5.2 is to be contrasted with the s.e. of 2.2 for IQs below 70 IQ. This finding, namely, that low IQs are more stable than high is not only statistically valid, but is in agreement with the experience of clinical workers. While the s.e. of an IQ is not large in view of the difficulties inherent in mental measurement, one can expect variations of as much as  $\pm 15$  points in IQ determinations of a bright child—due entirely to errors of measurement. For this reason, in part, much misunderstanding might be avoided if psychologists would describe a child by such phrases as "bright," "dull," and the like, instead of labelling him with an IQ of 82 or 112. Numerical scores convey an unwarranted impression of exactitude, and even small changes in them provoke criticism and misunderstanding.

Chapters VII and VIII deal, respectively, with the problem of scatter in IQ distributions and with percents passing the test items by age levels. Spread or scatter was found to be in part a function of recurring scale items, *i.e.*, of sub-tests placed at more than one age level. Scatter is not a consistent function of the individual and is not related to general IQ level. Tables give in full the percents passing the items by age groups. These data show a much higher percent passing at lower than at upper age levels. For example, 72% (on the average) of 4 year olds pass the items assigned to Year IV, while 60% of 10 year olds pass the items at Year X. Various psychometricians, as our author notes, have held that an item is correctly placed when assigned to that age level at which just 50% of the subjects pass it; and mathematically this would seem to be reasonable. McNemar, however, vigorously condemns reliance upon the 50% criterion. He lists three "considerations" (p. 87) to explain why higher percents are necessary in the New Revision. These are (1) "the fact that it is simply impossible otherwise to construct an age scale of the Binet type that will yield mean mental ages equal to mean chronological ages"; (2) "the fact that the location and grouping of items at a given level is mainly one of convenience which facilitates testing

and scoring"; . . . and (3) "the fact that the individuals of any age group encounter items which are actually of 50% difficulty for their age group even though the items placed at their own age levels may be less difficult." It is not immediately obvious that either (2) or (3) of these considerations is directly relevant to the issue of test placement, while (1) is simply a flat pronouncement of what scale standardization may have shown to be true without showing why this is true.

Terman's explanation of the allocation of tests in the Old Revision is much more clarifying (*Stanford revision of the Simon-Binet scale*, 1917, pp. 154-155) than is the present discussion. The rapid rise of the age-progress curve in the early years and the resulting drastic changes in percents passing, plus the increase in spread at the later ages, are apparently the crux of the matter. From the data in Table 26 (pp. 89-98), one may readily demonstrate to his own satisfaction that a typical 4 year old and a typical 12 year old will test approximately "at age" on the New Revision. Essentially the allocation of test items to specific age levels boils down to a cut-and-fit process. The authors could have saved themselves much space by saying so at the outset.

A persistent problem which plagues the constructor of a general intelligence scale like the New Revision is that of whether the test is measuring a "unitary" ability; and if it is whether this ability remains constant over a wide age range. In Chapter IX, McNemar attacks this problem for the New Revision using the methods of factor analysis. Intercorrelations were calculated between the separate items from four-fold tables. Fourteen separate factor analyses were then made (using Thurstone's centroid method), the test items being so dove-tailed as to provide a series of overlaps; e.g., the items of years 7 and 8 were combined in one analysis and the items of years 8, 9, and 10 in another analysis. Several facts stand out clearly in these tables, which may be summarized as follows. First, a strong first factor is conspicuous in all 14 tables. This factor accounts for the intercorrelations of the items and for from 35-50% of the variance of the battery. A study of overlap provides good evidence that the first factor weights are estimates of an ability which is consistent throughout the scale. McNemar points out that *some* generality is to be expected in the New Revision because the items were so chosen as to be correlated. And that the size of the first factor loadings are definitely limited by the low reliabilities of the separate items

(estimated to be around .65). Secondly, a second and perhaps a third independent factor may be present at the very early and at the later age levels. Rotation of the centroid axes gives a suggestion of additional factors, probably involving verbal and motor abilities at ages 2-2½; and of verbal and numerical group factors at age 5. But McNemar stresses the dubious nature of these additional factors. Thirdly, belief in the existence of a broad intellectual factor which runs through the New Revision is strengthened by the factor analyses. Test items having high loadings in this factor require, in general, ability to solve problems involving words, numbers, spatial concepts. Test items having low factor loadings are more concrete and manipulative and present tasks demanding somewhat routine activities. The fact that such a diversity of test items can be assembled to yield a general factor—even when carefully chosen so to do—is strong evidence (it seems to the reviewer) for the existence of such a general factor in mental activity as Spearman has long urged.

Chapter X presents three scales each made up of items selected from the New Revision. The first, a vocabulary scale, should prove useful as a quick estimate of general intelligence for the ages 8 to 18. Its correlations with Stanford-Binet MA are .71, .83, .86, and .83 for ages 8, 11, 14, and 18. The second scale is non-verbal. There are two forms of this test each containing one-half of the 40 items judged to be the least verbal in Forms L and M. These scales are less highly saturated with the general factor than are the other items in Forms L and M. They correlate around .65 with MA. A memory test, consisting of two forms each of 22 items makes up the third scale. Correlations of this test with MA show these items to be measuring much the same ability as the scale as a whole. This is not surprising in view of the evidence that memory and general intelligence (as measured by the Binet type of test) are not different functions at low age levels. Memory does appear to be a function distinct from intelligence at later ages and is certainly so at the college level.

The first ten chapters of this book are concerned primarily with the standardization data of the New Revision. In Chapter XI (*Units of Measurement*), McNemar drops the role of expositor to express positive opinions upon several of the persistent problems which confront the test-maker. McNemar considers first the question of whether standard scores furnish units which are somehow more fundamental and more "truly equal" than are original



scores. A standard score, he writes (p. 158), is simply a linear transformation of raw scores into SD-units; such a transformation (he points out) does not change the form of the distribution nor equalize the units of the test. This is entirely true, but is not directly relevant to the problem of scaling test scores into "truly equal units" (the phrase is Thorndike's). I cannot agree with McNemar that there are "a surprisingly large number of psychologists 'who claim' that the use of the standard-score method will yield units which are equal or 'truly equal' (p. 157)." When the normal curve hypothesis is tenable (or distributions are of the same form), standard scores are convenient devices for comparing or combining tests scored in different or in incommensurable units. Thus, on the assumption of normality of distribution, a ten year old boy who is 1 SD above the mean of his age group for height, and 1 SD above the mean of his age in weight is, in a real sense, as heavy as he is tall. But the transformation of obtained scores into standard-scores makes no assumption in itself of normality, nor does such transformation lead to units which are any more nearly equal than are the raw score units.

I think that McNemar has confused standard-scores with T-scores,—at least so far as equal unit scaling is concerned. T-scaling converts the percents achieving each score into equivalent SD-positions along the baseline of the normal curve. No assumption is made regarding the distribution of raw scores, but the true distribution of ability measured by the test is assumed to be normal. The distribution of calculated T-scores will have the same form as that of the original scores; only in the ideal normal distribution assumed for the trait are the T-scores normally distributed. SD-units along the baseline of the normal curve (upon which our T-scores will fall at irregular intervals) are mathematically equal or in Thorndike's words "truly equal."

The point should be stressed, perhaps, that the units on *any* scale are equal *only* with respect to certain defined criteria. Psychologists have done too much breast-beating over the great advantage possessed by the physicist in having scales with equal units and a true zero. To be sure, the much-vaunted "cgs" scale does possess many advantages, among them that of having equal units; but these units are equal with respect to certain criteria,—not all. The Weber-Fechner Law has shown that an inch added to a man's nose is not *perceptually* the same as an inch added to his height, and the same number of pounds or of seconds is often

psychologically quite unequal. The same situation holds even more truly of other physical scales. The thermometer measures temperature changes by the height of a column of mercury. Divisions on the scale are physically equal. But these equal scale units reflect equal temperature changes in a specific rather than in a general sense. The amount of expansion or contraction of a physical body can perhaps be predicted from a knowledge of thermometer reading changes. But it is doubtful whether such changes represent equally well perceived differences in the resistance properties of a body, tensile strength, or in the human organism. McNemar (p. 159) in his example showing that pounds are unequal to T-scale units exhibits clearly the confusion which arises when systems of measurement are compared which are set up on different criteria and are not directly commensurable. The scaled scores of the psychologist are "truly equal" with respect to a defined criterion of performance on the assumption of normality in the trait. That such scales are useful and efficient experience has amply shown.

Closely related to his discussion of equal units is McNemar's criticism of Thorndike's demonstration that intellect in man is normally distributed. McNemar contends that since Thorndike scaled his tests into equal units by means of the normal curve, it is not surprising that distributions expressed in these derived units should turn out to be normal. He writes: (p. 17) "Thorndike has only demonstrated the obvious; a normal curve can be produced by assuming it in advance." It may be noted that Terman has also made essentially the same criticism (*Measuring Intelligence*, p. 25): he calls it "lifting oneself over the fence by one's bootstraps."

This criticism, I feel, is not only superficial and incorrect, but coming from such sources is liable to do real harm. It may be pointed out, in the first instance, that Thorndike fully understood the "circularity" objection advanced by McNemar. In Chapters II, VIII, and Appendix III of his *Measurement of Intelligence* (1927), he states explicitly that a normal distribution of test scores might arise in lieu of other causes from (1) the fact that the test was constructed with the normal hypothesis definitely in mind; and (2) from chance errors, the scores of all subjects being the same. To meet the first difficulty, Thorndike employed two procedures. First, he scaled the score distributions for several well known tests (Otis, Alpha, National, are examples) into "truly equal" units by means of the normal curve. The normal distribu-

tion was selected for scaling (others were considered, see Chapter II) because the distributions of scores in the school grades employed as standardization groups were bell-shaped and not noticeably skewed. Distributions of scores, expressed in terms of corrected units, were then demonstrated to be normal for new and different groups taken at various age levels. It is important to note that the scaling procedures corrected the units of the test (lengthened or compressed them) but did not *force* the new distributions into the normal form. As a second procedure, Thorndike showed that a test scored in truly equal units derived from Grade VI will return a normal distribution for Grade IX. He argues that while a test-maker might conceivably select his items so as to return a normal distribution for a given group, it is too much to expect him to repeat for other and quite different groups. To meet the second difficulty, Thorndike averaged four to five repetitions of the same test before scaling, on the hypothesis that the mean of several determinations would be largely free from chance error.

One may offer valid objections to certain details of Thorndike's procedure. I am not entirely convinced that he has really demonstrated intellect to be distributed in strictly normal fashion, and I agree with McNemar that his use of the Chi-Square test is dubious. But I feel sure that Thorndike has shown intellect in man (as measured by standard tests) to be essentially symmetrical and bell-shaped, and at least to be approximately normal. And this demonstration is not invalidated by the rather shallow charge of circularity.

In summarizing the material in this monograph, I should like to express the opinion that the New Revision is the most useful and is certainly the best constructed instrument for measuring the intelligence of children which we now possess. It represents an achievement of first rank; and one of which all psychologists, no matter what their persuasion, may well be proud.

The first of these is the fact that the United States is a young nation, and that its history is a history of growth and expansion. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one. The second is the fact that the United States is a nation of immigrants, and that its history is a history of the struggle for the rights of these immigrants. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one. The third is the fact that the United States is a nation of free men, and that its history is a history of the struggle for the rights of these free men. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one.

The fourth is the fact that the United States is a nation of law, and that its history is a history of the struggle for the rights of these laws. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one. The fifth is the fact that the United States is a nation of progress, and that its history is a history of the struggle for the rights of these progress. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one. The sixth is the fact that the United States is a nation of peace, and that its history is a history of the struggle for the rights of these peace. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one.

The seventh is the fact that the United States is a nation of justice, and that its history is a history of the struggle for the rights of these justice. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one. The eighth is the fact that the United States is a nation of freedom, and that its history is a history of the struggle for the rights of these freedom. It is a history of a people who have been able to overcome the difficulties of a new and untried land, and who have been able to build a great and powerful nation out of a small and weak one.

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## PSYCHOLOGY AND THE WAR

Edited by

STEUART HENDERSON BRITT

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CLEVELAND COMMITTEE FOR DEFENSE



# THE HISTORY OF THE WORLD

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## PERSONNEL RESEARCH IN THE ARMY II. THE CLASSIFICATION SYSTEM AND THE PLACE OF TESTING

BY STAFF, PERSONNEL RESEARCH SECTION, CLASSIFICATION  
AND REPLACEMENT BRANCH, THE ADJUTANT  
GENERAL'S OFFICE

A previous article (1) surveyed the organization, staff, and field of responsibility of the Personnel Research Section of the Classification and Replacement Branch, The Adjutant General's Office, War Department. This is the agency charged with the construction of tests used in Army general classification and special selection, and with the preparation of various other psychological tools used in Army personnel work.

The present article will consist of a brief résumé of the Army classification system, the part which psychological testing plays in it, and a list of tests already developed by the Personnel Research Section. The intention here is to provide a general picture of the Section's work; detailed reports on the construction and use of individual tests or on other current classification projects will be published by the Section from time to time.

Classification is a complex and continuing process, coextensive with a man's entire military career. The devices and aids which the Personnel Research Section develops are employed by the officers and enlisted men of the Army's personnel system in classifying and assigning the individual soldier from the time of induction until he leaves the Service, possibly as a commissioned officer. A man may advance in grade or revert to a less responsible job after failure to measure up; careful record is kept of each significant change in status or duty. The accompanying flow chart (Fig. 1) indicates the major steps in Army classification.

The officers who administer the program in the field are, almost without exception, trained personnel technicians who have in addition attended courses in Army classification taught since 1941 at The Adjutant General's School at Fort Washington, Maryland. They must be able to direct administration and scoring of tests, and to interpret test results. These officers are chosen from men who enter the Army with a background of training and experience,

in psychology or personnel practices. The enlisted men who assist them are detailed to this work because of some training, experience, or interest in personnel problems. The work of the classification officers and men is supplemented by and checked with the judgments of line officers, who are asked to rate the men assigned to their command on the basis of actual performance of military

#### FLOW CHART—ARMY CLASSIFICATION SYSTEM

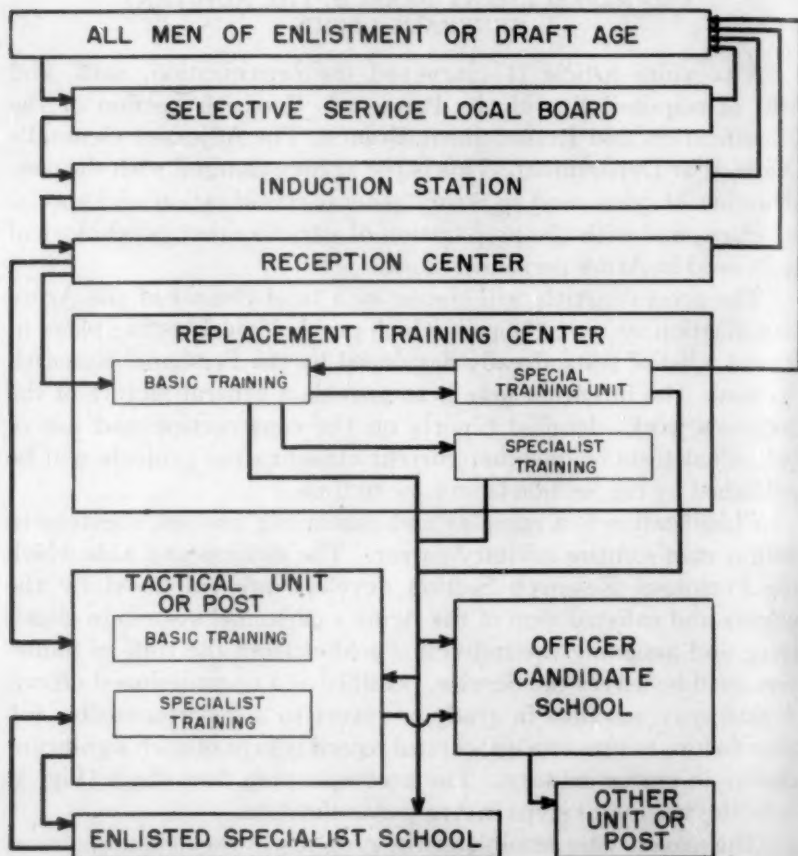


FIGURE 1

This chart is intended to illustrate only the major agencies and organizations through which a man may pass or to which he may be assigned in the process of Army classification. Military necessity, local conditions, or special requirements of some arms or services make it impossible to show more than the usual stages involved.

duties. Every effort is made to see that all measures of a man's qualifications are taken in a fair and objective manner.

The important early steps in classification occur at the Induction Station, the Reception Center, and the Replacement Training Center. At the Induction Station, before the registrant or recruit actually becomes an enlisted man, he will, if his literacy is in doubt, be given a self-administering minimum literacy test. This very simple test is intended to discover those men unable to read or write at fourth grade level, and who would therefore have difficulty in absorbing Army training, a large part of which is based on manuals and other written material. Men failing the minimum literacy test are given a Visual Classification Test which serves to indicate to the examiners those men who, although illiterate or non-English reading and speaking, are sufficiently high in native ability to learn military duties. Some of these men, if inducted, may later be sent to special training units where they will receive elementary instruction in reading and writing. According to present regulations, no more than ten percent of the white and ten percent of the colored men inducted in a single day at any one station may be illiterates.

The enlisted man is next sent to one of the more than thirty Reception Centers in the United States, where the major part of initial classification is accomplished. At the Reception Center all literate men are given the Army General Classification Test (GCT) which yields a rough estimate of the individual's relative ability to learn. The standard score distribution on this test is divided into five Army grades indicative of learning capacity: I—very rapid learners, II—rapid learners, III—average learners, IV—slow learners, and V—very slow learners. The number indicating an individual's grade is entered on the Soldier's Qualification Card which will eventually contain all essential information about the enlisted man's background and Army career and which will follow him and be kept up to date wherever he may be assigned.

Two other tests are given at present to all men who speak and read English, during their several day's stay at the Reception Center: the Mechanical Aptitude Test and the Radio Telegraph Operator Aptitude Test. These two tests assist in the selection of men who may profit most from specialist training in mechanics or in radio-telegraphy, vital fields in which the Army cannot expect to enlist enough men already trained or experienced. Scores for these tests are entered, along with that for the General Classification Test, on the Soldier's Qualification Card.

Perhaps the most important single step in Army classification is the interview at the Reception Center, in which a trained Army interviewer obtains the information required on the Qualification Card on the basis of an informal discussion with each enlisted man. These data include such items as education, languages, main and second best occupations, job history, hobbies, leadership experience, and previous military training. The test scores, already recorded on the Card, may assist the interviewer in assessing a man's qualifications; they will also assist the classification officer who uses the Card later as the basis for making an initial assignment.

If the man possesses skills immediately useful, or if he is needed at once to fill a division, he may be sent directly to a unit where basic training will be given along with training for a particular Army job. Other men, about fifty percent at present, are sent to a Replacement Training Center of one of the branches of the Service, such as Engineers, Infantry, or Signal Corps. At these Replacement Training Centers the enlisted man receives basic training and learns the duties he needs to know in order for him to take his place in the ranks of that particular branch.

If the officers in charge of classification at the Replacement Training Center have reason to believe that the low score made by a man on the GCT was due to language difficulty and does not fairly represent his ability, he may be given a non-language test in which a minimum of language skills is involved, instructions being given in standardized pantomime. Scores for this test are converted to Army grades exactly as are those for the GCT.

Throughout his service, the soldier will be rated or selected for special duties by the classification officers and line officers of the units to which he may be attached. Many men must be chosen on the basis of test scores, background, or performance to attend enlisted specialist schools where technical training is given in skilled Army occupations. Others, whose test scores indicate high intelligence and who demonstrate their ability to lead, will be sent to Officer Candidate Schools from which they will graduate as commissioned officers. Still other men who fail to learn their duties readily because of mental inadequacy, emotional instability, or similar reasons, may be sent to the Special Training Units where various types of elementary instruction will help them adapt to the Service. Some will be found altogether unfit and will be sent back to civilian life. The conditions under which the tests are admin-



istered limit the types which are useful in military classification. In general, Army tests must be objective, specific, suitable for group administration, and relatively simple to score and interpret.

Nearly all tests which the Section has developed may be scored either by hand or by machine; raw scores are usually convertible to standard scores (based on a mean of 100 and a standard deviation of 20) and to Army grades.

A list of tests developed to date is appended, together with brief comments on several of the more important and typical examples. Also included is a chart (Fig. 2) which shows customary procedures in construction.

#### TESTS DEVELOPED

##### *Classification Tests*

- General Classification Test
- Non-Language Test
- Visual Classification Test
- Higher Examination
- Officer Candidate Test
- Women's Classification Test (Mental Alertness Test)
- Army Information Sheet (Minimum Literacy Test)

##### *Aptitude Tests*

- Mechanical Aptitude Test
- Clerical Aptitude Test
- Radiotelegraph Operator Aptitude Test
- Code Learning Test
- Battery of Tests for Combat Intelligence
  - Identification of Aerial Photographs
  - Map Identification
  - Route Tracing
  - Battle Maps
  - Perception of Detail
  - Map Reading
  - Map Orientation

##### *Educational Achievement Examinations*

- Algebra
- Arithmetic
- English Grammar and Composition
- French
- General History
- German
- Inorganic Chemistry
- Physics
- Plane and Solid Geometry

## DEVELOPMENT OF TESTS FOR CLASSIFICATION PURPOSES

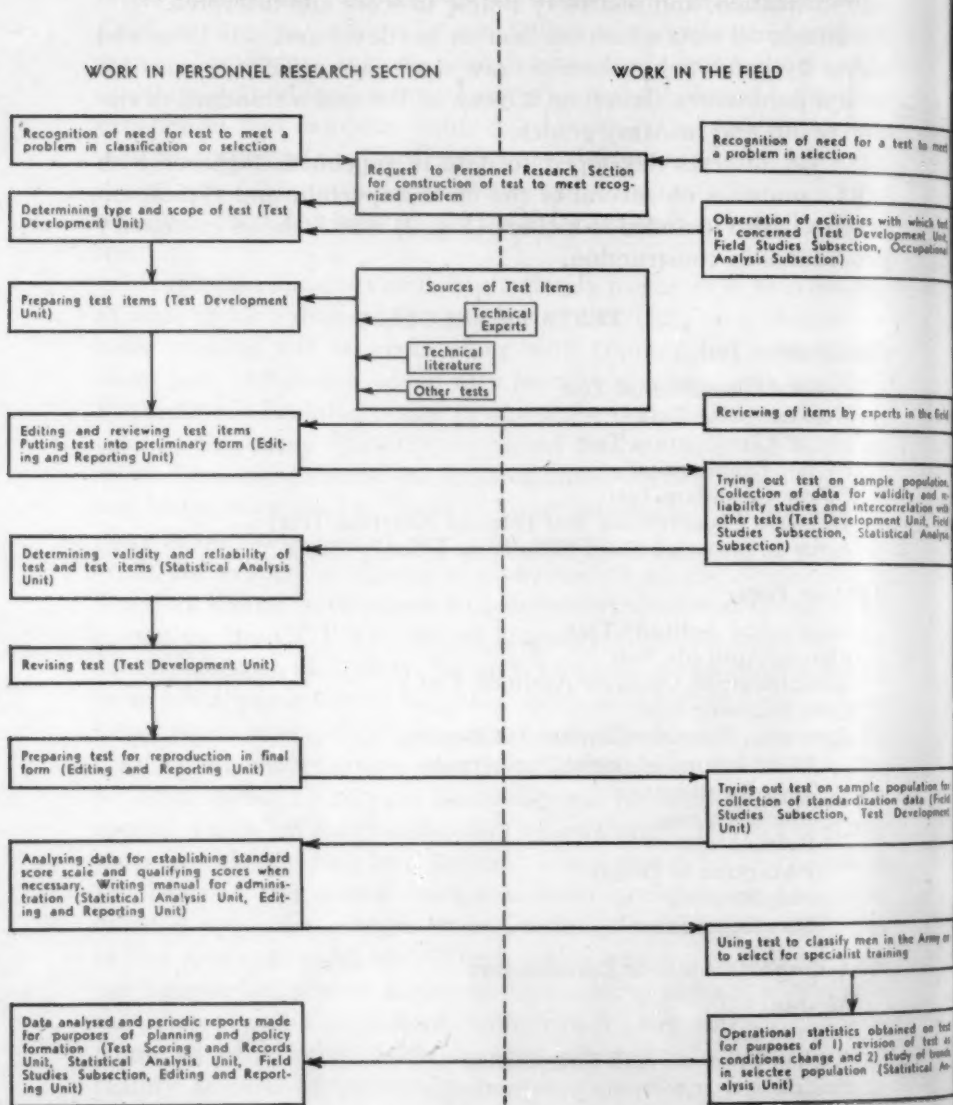


FIGURE 2

Spanish  
Trigonometry  
United States History  
Combined Algebra, Trigonometry, and Geometry

#### *Trade Knowledge Tests*

General Automotive Information Test  
General Electricity and Radio Information Test  
General Electrical Information Test  
General Radio Information Test  
Driver and Automotive Information Test

#### *Warrant Officer Examinations*

About 30 technical examinations

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## UTILIZATION OF CLINICAL PSYCHOLOGISTS IN THE GENERAL HOSPITALS OF THE ARMY

BY JAMES W. LAYMAN

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Previous articles by Bingham (1), Britt (2), Flanagan (4), Jenkins (5), Louttit (6), Pratt (8), Seidenfeld (9), Wolffe (10), and others, have presented information relating to the war effort and the uses to which psychologists are being put. More recently several clinical psychologists were commissioned through the Surgeon General's Office, United States Army, for service in the neuropsychiatric sections in certain of the General Hospitals.

The names and qualifications of those selected were obtained from the National Roster of Scientific and Specialized Personnel some time during 1941. At that time the Office of Psychological Personnel had not yet been established, but contacts were made with this office after it came into being, shortly before the commissions were granted. Correspondence with those on the original list was initiated through Lt. Colonel P. S. Madigan of the Surgeon General's Office during the spring of 1942. Six psychologists were commissioned in the early summer as First Lieutenants in the Sanitary Corps, the unit of the Medical Corps which comprises chiefly the non-medical specialists in the Medical Department. The following are those commissioned:

M. Dunn, Darnall General Hospital, Danville, Kentucky  
R. M. Hughes, Lawson General Hospital, Atlanta, Georgia  
J. W. Layman, Walter Reed General Hospital, Washington, D. C.  
W. C. Murphy, Letterman General Hospital, San Francisco, California  
L. I. O'Kelly, Fitzsimmons General Hospital, Denver, Colorado  
L. G. Tennes, Brooke General Hospital, Fort Sam Houston, Texas

The Surgeon General's Office did not specifically define the nature of the assignments to those commissioned. Instead the appointments appear to be an experimental attempt to determine whether or not clinical psychologists can be utilized in the Neuropsychiatric Sections, and if so in what ways. This has permitted the medical officer in charge of the Neuropsychiatric Section to regulate and define the duties. It also has permitted each of the psychologists to seek his own level within the limitations imposed

by his particular training, experience, and other qualifications. That this was a wise decision is attested by reports from the psychologists commissioned. Although the nature of the duties assigned varies somewhat, all report a favorable reception. As yet there has been no official evaluation of their contributions and services.

To understand the work duties, it is necessary to discuss briefly the general function of the Medical Corps, especially the Neuropsychiatric Service. At the point of induction or enlistment, the Medical Corps shares with other units the responsibility of selecting those most fitted for military service. These requirements, in general terms, are expressed as follows (3):

The army is one of the elements of national defense and its present mission is one of preparation for an offensive-defensive type of warfare. It is in no sense a social service or a curative agency. It is to be considered neither a haven of rest for the wanderer or shiftless nor a corrective school for the misfits, the ne'er-do-wells, the feeble-minded or the chronic offender. Furthermore, it is neither a gymnasium for the training and development of the undernourished or undeveloped, nor is it a psychiatric clinic for the proper adjustment to adulthood emotional development. Therefore, there is no place within the army for the physical or mental weakling, the potential or prepsychotic or the behavior problem. If an individual is a behavior problem in the civilian community he will (almost)\* certainly become a more intensified problem in the army.

Despite rigorous efforts at prevention, it is inevitable that some persons with the above mentioned physical, personality, or behavior deviations do get past the preliminary screening processes. Also, such casualties do develop as incidental to the service. With reference to the psychoneurotic and psychotic cases, Porter, Novak, and Lemkau (7) point out that "recruits are constantly under the supervision of experienced non-commissioned officers who are adept at picking out men showing any deviation from normal." Whether these casualties develop as incidental to the service or are recognized after induction as a result of the aforementioned close supervision, the Army has made provisions for their study, treatment, and disposition along prescribed channels.

Along with other responsibilities, it is the task of the Neuropsychiatric Section to look after those cases which demonstrate psychoneurotic or psychotic reactions. While other administrative arrangements exist with reference to disposition and treatment of

\* The word "almost" is the writer's. It is added since there is some evidence that certain deviants can and are adjusting to military demands.



the psychopath, in actual practice they frequently are referred to the Neuropsychiatric Section. Regardless of the terminal diagnosis of the cases referred, a longitudinal and cross-section study is made of each. As aids to the longitudinal studies, case histories secured and prepared by competent social agencies and reports from the soldier's unit commander are obtained. The reliability of this aspect of the study is furthered by careful observation of the soldier's adjustment to ward routines as well as his reactions and relations to his fellow patients. The cross sectional study consists of frequent interviews in an effort to understand the dynamic factors which contributed to and now operate to effect the behavior observed. As can be seen, then, the Neuropsychiatric Section is responsible for the determination of whether or not some form of mental, emotional, or other abnormality exists in the patients. Cases referred can in general be said to include all the syndromes classified under the various forms of psychoneuroses and psychoses and other types of maladjustment. As already indicated, the psychopathic personalities need not necessarily come under the purview of the psychiatrist. This is true also of those sufficiently feeble-minded as not to be adaptable to the military service. They must be disposed of under the provisions and regulations which govern them, when referred to the Neuropsychiatric Section.

There are certain other duties to which any commissioned officer of appropriate rank might be assigned regardless of his professional background. These duties are chiefly of an administrative nature, and include taking turn as administrative officer-of-the-day, and serving as a member of various boards and courts. As would be expected, each of the psychologists commissioned has been assigned to one or more of the administrative details to which he is eligible as an officer.

Analysis of the reports received from those commissioned shows that they have been responsible for the administration of various tests to cases referred from the Neuropsychiatric Section and from other wards of the hospital. This responsibility in itself represents a forward step in that it has placed the test administration into more competent hands; testing previously done had been handled by persons without particular training in this field. In addition, it has been possible to add tests which had not been used previously and which were more appropriate to problems raised by particular diagnostic problems. Thus, the Wechsler-Bellevue (Army revision), Arthur performance, Babcock-Levy test of men-

tal functioning, Rorschach, and others, are being used where indicated.

Beyond the common function of psychometrics, professional duties tend to vary considerably and include such activities as liaison with Red Cross case workers in securing social histories, assistance with electroencephalographic studies of certain cases, classification of officer personnel, examination of mental status of patients, supervision of occupational and recreational therapy, and preparation of case summaries for staff diagnosis.

The time spent in these various activities varies considerably. One psychologist officer indicates that he spends approximately one-half of his time on psychometrics and the other half on various administrative duties. Another states that about seventy-five percent of his time is occupied in the Neuropsychiatric Section (psychometric testing and diagnostic studies of mentally deficient, psychoneurotic, and psychopathic cases) and the remainder in classification of officer personnel. One psychologist who reported for duty several months later than the others indicates that he is still in the process of orientation and that his duties are not too well defined except for the psychometric responsibility. Still another, aside from his administrative assignments, gives most of his time to duties similar to those performed by the psychiatrist; he makes the complete study of cases assigned to him (except for the physical and neurological or any other strictly medical examinations) and follows them through to their completion.

Opportunities for research are almost unlimited. From a military standpoint, one of the most significant problems relates to selection of soldier and officer material, i.e., the differential factors concerning those who do and do not adjust to military requirements. Also, there is much that can be learned concerning the dynamics underlying the symptomatology of those who have not adapted; and this in turn should be of value for its mental hygiene applications to non-military life.

To what extent the nature of the professional duties assigned has been determined by such factors as the newness of psychologists in these hospitals and consequent need for a period of mutual adjustment, experience of those commissioned, attitude of those in charge, exigencies of the situation prevailing in specific hospitals and others, it has not been possible to determine. Judging from the reports received, each psychologist now commissioned indicates that his work is being favorably received. In fact, one psy-

chiatrist indicates that such an appointment is "making history" and that he is very pleased to have a psychologist on his staff. Another psychiatrist says that his only complaint is "that our psychological staff is too small to get all the service from it that we know such a department is capable of giving."

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## CIVILIAN ASSISTANCE TO MILITARY PSYCHOLOGISTS

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One of the functions of the Emergency Committee in Psychology of the National Research Council has been to help psychologists in the armed forces or in other governmental services to find assistance for their research, and to keep directors of university laboratories informed, whenever and wherever possible, of the kind of research which would presumably be of most value to the war effort. These purposes have not been easy to fulfil, although in a number of instances it has been possible to arrange for profitable exchange of ideas between military and civilian psychologists.

Most of the research being done for the government is supposed to be confidential in nature. It is therefore awkward for men in charge of this research to farm out some of their problems to civilian psychologists, much as they would like to do so. And it has been especially embarrassing to confront civilian psychologists, eager to offer their services, with the statement that no specific directives regarding research in military psychology could be given out. This problem came up for frequent and prolonged discussion during the early meetings of the Emergency Committee. At one meeting it was suggested that Dr. Dael Wolfe and the writer draw up a list of research topics in psychology that would have a more or less direct bearing on the war effort. This list of topics was preceded by the following paragraph.

The tremendous changes made necessary by the war offer many opportunities for American psychologists to engage in study and research of great military and social value. Some of these opportunities involve problems for which immediate answers are urgently needed; others, not so immediately urgent, will prepare for the social problems of post-war readjustment. In working toward solutions of some of the problems the psychologist can proceed by himself. A good many important problems, however, cut across disciplinary boundaries and demand the cooperative efforts of men and women from several different fields of research in the natural and social sciences. Not all of the problems can be solved by 'pure research'; many require a marshalling of whatever evidence can be

found, followed by the exercise of the best scientific, social, and practical judgment available. Some of the problems are particularly suited for seminar discussion and for group recommendation.

Nearly seventy different topics were listed under such headings as sensory and perceptual capacities, problems of selection of personnel, learning and training, morale, and post-war readjustments. In considering these topics the Committee soon realized that almost every problem that psychologists might care to investigate could be thought of as having some bearing on the war effort. Many of the results of investigations into morale, for example, which Dr. Gordon W. Allport secured from seminars in different universities have found useful application in various governmental agencies. Since it was difficult to secure and give clear-cut directives to civilian research, it was therefore decided to encourage psychologists to continue their work in laboratories, libraries, and seminars under self-imposed directives, for at the most unexpected moment the results of their inquiries might be needed and used. A note to this effect was published in the *Psychological Bulletin* (1).

The large number of psychologists now employed by the armed forces and by various governmental agencies makes the need of civilian assistance less urgent than it was a year or so ago, and the number of civilian psychologists who can render such assistance is becoming steadily smaller. With the approach to total war, however, the situation may change in unpredictable ways. Civilian psychologists now engaged in teaching, and therefore unable to devote time to research, may find their courses and students drastically reduced in numbers. In order that there may be no unnecessary loss in efficient prosecution of psychological warfare, the Emergency Committee, either through the writer or through the Office of Psychological Personnel, 2101 Constitution Avenue, Washington, D. C., is eager to be kept informed of available facilities which might, if the occasion arises, be used for the study of problems which originate in the armed services and which can be legitimately turned over to civilian psychologists.

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## REVISION OF SELECTIVE SERVICE OCCUPATIONAL BULLETIN NO. 10

Psychology was one of the "critical occupations" listed in Selective Service Occupational Bulletin No. 10, issued June 18, 1942. (See *Psychological Bulletin*, 1942, 39, 525-528). This Bulletin was revised on December 14, 1942, with the addition of industrial engineering, sanitary engineering, and bacteriology, and with the exclusion of accounting, economics, industrial management, personnel administration, and statistics. In other words, psychology is one of eight specialized fields *in addition to engineering subjects* now listed as having "serious shortages of persons trained, qualified, or skilled to engage in these critical occupations":

### *Engineering Fields*

- Aeronautical Engineers
- Automotive Engineers
- Chemical Engineers
- Civil Engineers
- Electrical Engineers
- Heating, Ventilating, Refrigerating, and Air Conditioning Engineers
- Industrial Engineers
- Marine Engineers
- Mechanical Engineers
- Mining and Metallurgical Engineers, including Mineral Technologists
- Radio Engineers
- Safety Engineers
- Sanitary Engineers
- Transportation Engineers—Air, Highway, Railroad, Water

### *Other Specialized Fields*

- Bacteriologists
- Chemists
- Geophysicists
- Mathematicians
- Meteorologists
- Naval Architects
- Physicists, including Astronomers
- Psychologists

The following sections are quoted from revised Occupational Bulletin No. 10:

#### *4. Deferment of students in training and preparation*

A registrant who is in training and preparation for one of these scientific and specialized fields may be considered for occupational classification as follows:

(a) A registrant in training and preparation in one of the Engineering Fields may be considered for occupational classification after completion of his first academic year in a recognized university or college, and thereafter, if he is a full-time student in good standing, if he continues to maintain a good standing in such course of study, and if it is certified by the institution that he is competent and that he gives promise of successful completion of such course of study and acquiring the necessary degree of training, qualification, or skill.

(b) A registrant in training and preparation in one of the Other Specialized Fields may be considered for occupational classification after he has reached the point in such course of study in a recognized university or college, and thereafter, where there remains not more than two academic years for him to complete such course of scientific and specialized study, if he is a full-time student in good standing, if he continues to maintain good standing in such course of study, and if it is certified by the institution that he is competent and that he gives promise of successful completion of such course of study and acquiring the necessary degree of training, qualification, or skill.

#### *5. Graduate Students*

A graduate or postgraduate student undertaking further studies in these scientific and specialized fields following completion of his normal undergraduate course of study may be considered for occupational classification if, in addition to pursuing further studies, he is also acting as a graduate assistant in a recognized university or college. A graduate assistant is a student who in addition to pursuing such further studies is engaged in one of the following:

(a) In scientific research certified by a recognized federal agency as related to the war effort; or

(b) in classroom or laboratory instruction for not less than twelve hours per week.

#### *6. Opportunity to engage in profession*

When a registrant has completed his training and preparation in a recognized college or university and has acquired a high degree of training, qualification, or skill in one of these scientific and specialized fields, such registrant should then be given the opportunity to become engaged in the practice of his profession in an activity necessary to war production or essential to the support of the war effort. In many instances following graduation from a recognized college or university, a certain period of time will be required in the placing of trained, qualified, or skilled personnel in an essential activity. When a registrant has been deferred as a necessary man in order to complete his training and preparation, it is only logical that his deferment should continue until he has had an opportunity to use his scientific and specialized training to the best interest

of the nation. Accordingly, following graduation from a recognized college or university in any of these scientific and specialized fields, a registrant should be considered for further occupational classification for a period of not to exceed 60 days in order that he may have an opportunity to engage in a critical occupation in an activity necessary to war production or essential to the support of the war effort, provided that during such period the registrant is making an honest and diligent effort to become so engaged.

#### 7. *Effective period of this bulletin*

This bulletin and attached list amends and supersedes Occupational Bulletin No. 10 of June 18, 1942. This amendment is effective until July 1, 1943, unless sooner amended. During the effective period of this bulletin the War Manpower Commission is giving further study to the training and preparation and utilization of persons trained in these scientific and specialized fields.

### PSYCHOLOGY AND THE WAR: NOTES

*Cleveland Committee for Defense.* Dr. Clifton W. Hall, President of the Cleveland Association for Applied Psychology, appointed a Committee for Defense in September, 1941, comprising the following members: Jay L. Otis, Myra E. Hills, Arthur T. Orner, Agnes H. Reigart, and Rosina M. Brown, Chairman.

At the outset it was decided to get as much information as possible from those countries already at war. Newspapers and periodicals were culled for articles on mental and emotional reactions due to the war. Bulletins from the British Library of Information, the Anna Freud News Letters from Hampstead Nursery, and the various surveys made by the British authorities on the effects of evacuation, foster home placement, etc., were excellent sources. The United States Department of Labor also loaned a collection of photostat copies of foreign publications which yielded much information and advice. Abstracts of pertinent articles were made and filed at the Board of Education Branch of the Cleveland Public Library and made available to the community. This material has been used as a basis for talks by various persons dealing with the welfare of the child in wartime.

It was also felt that there was a need for a brief, simple pamphlet which could be distributed to the homes in this industrial community. Although some articles on the effect of war on children have appeared in the local newspapers, many homes are not reached by these papers. Many parents are of foreign extraction or of little schooling. The Committee thought this an opportunity to bring to these parents some general principles of child psychology in simple English. The resulting pamphlet, "Your Family and You," was distributed through the schools and made available to the various community agencies. The Cleveland Board of Education believed the project to be of sufficient value to underwrite the publication.

## BOOK REVIEWS

BORING, E. G. Sensation and perception in the history of experimental psychology. New York: D. Appleton-Century, 1942. Pp. xv+644.

The present book is, in the author's plan, the second in a series of three volumes on the history of experimental psychology. His original plan, formulated in 1924, envisaged a volume which would start with a survey of men and schools, and then trace the history of experimentation and thought in the fields of sensation, perception, feeling, emotion, learning, memory, attention, action and thought. But the project expanded to the point where the author decided to publish a separate book on men and schools alone, which appeared in 1929 under the title, *A History of Experimental Psychology*. The criticism, frequently heard that this original work belied its title, because it failed to emphasize the experimental topics as much as it stressed movements, is now answered by the author. He never intended it as a complete coverage, but only as an introduction. This second volume, which covers the fields of sensation and perception, is much more truly a history of experimentation. The promised third volume, which is to cover the remaining fields, will round out the historical picture of what is traditionally understood to constitute the domain of orthodox, experimental psychology, that is, the range of human, adult, normal, general experience and behavior. It is to be hoped that the author will persevere in his endeavor.

The present book forms a natural unit of subject matter, since it is only by a process of artificial abstraction that such a topic as color is assigned to the subject of sensation, while movement is relegated to that of perception. Historically, also, these two topics have developed together; and, except for a liberty which the author has purposely taken, namely, the subordination of historic importance to ultimate significance, an even greater share of the three-volume series might have been devoted to sensation and perception. Fortunately, a great deal of sifting has been done, and the remaining well-screened material has been deflected into a few major hoppers. All topics have been subjected to the following test: "If any event important in the past has no demonstrable indirect effect upon the present, then it should be omitted from a book that tries to recreate the past merely to explain the present." Selah!! And, let it be added, if any event of transcendent importance in the past has a relatively minor effect on the present, then it should be shrunk to a space appropriate to its present and probable future significance. This places the writer of this type of history under a tremendous obligation to overcome biases, see beyond his own interests, and indulge in ruthless surgery. On this basis, however, the author has compressed the sacred cows of Nativism-Empiricism and Weber's Law into parts of chapters, where they occupy no more space than the discussion of such recent subjects as Gestalt dynamics.

The inevitable problem arises of where to end an historical account, especially of those topics which have but recently sprung into prominence. The author effectively spikes the critic's guns by declaring that "it has been my intention to slow down at 1920, and to stop about 1930 except when the momentum of discovery is irresistible as it was in the psychophysiology of hearing, when seventy-year-old problems began to come up for solution in the 1930's. Nevertheless the reader should not trust me after 1930, since I do not trust myself." Could he have been more adroit?

Realizing that a work of this sort may be used more for reference purposes than for continuous reading, Boring has introduced dates after the names of key men each time they have been mentioned, with the idea of orienting the reader with reference to other contemporary contributions. Thus E. H. Weber (1846) has a significance which is quite different from that of E. H. Weber (1852). Throughout the book, the reader feels that Boring has a well-thought-out philosophy about the history of a science; that he tries to psychologize, to look behind the works of authors into the motives which prompt them; in short, that he is engaging in what, to him, is a fascinating hobby. This impression is confirmed in the very last chapter of the book, which is devoted to the subject, "Concerning Scientific Progress." How, he asks, does scientific thinking get on, and, on the other hand, what holds it back? Why couldn't Charles Bell have formulated the resonance theory of hearing? He answers in terms of six principles of limitation: First, that scientific progress at any point waits on the discovery of instruments and techniques; second, that discovery is serial, presupposing other knowledge; third, that insight conforms to the *Zeitgeist*, rarely departing widely from contemporary thought; fourth, that men are hampered by their own habits of thought, overlooking that which contradicts these habits; fifth, that men's personal attitudes, their egoism and need for prestige divert their thought; and sixth, that social attitudes, the opinion of the in-group versus the out-group, as represented by schools, or national or regional groups, constrain the thought of the members of these groups.

After a preliminary discussion of the major problems of sensation and perception, such as attributes, nativism-empiricism, and the like, and a survey of the development of conceptions of the physiological basis of sensation, the author devotes six chapters to vision, including color phenomena and color theory, and the visual perception of bidimensional space, depth and distance. Then follow three chapters on audition, covering the psychophysics of tone, auditory perception, and auditory theory. Tactual and organic sensibility together occupy approximately as much space as audition, with smell and taste sandwiched into a twenty-page interlude as befits the uncertain state of the topic. A concluding chapter is concerned with the perception of time and movement.

In very few places can the book be said to express strong negative biases, but there are a few. One of them is in a direction for which the reviewer cannot offer a very strong protest, because he shares the same bias. Gestalt psychologists are classed as the modern nativists, because their "givens," like the *phi*-phenomenon, are not to be explained but



merely accepted. This is "faith and not a theory." They are correctly classed with the "phenomenologists" in contrast to the "experimentalists," although many might resent the implications of such a contrast. But the author can hardly be said to under-rate the school, since the closing words of the book read, "He who would understand the nature of the positive contribution of Gestalt theory to psychology needs no better tutor than this history of research and thought on the perception of apparent movement." Regarding modern fruitful adaptations of psychophysics, such as the "law of comparative judgement" and the psychometrics of testing, nothing is said; perhaps because these applications are not strictly relevant to perception, perhaps for different reasons. Again, one is slightly piqued to find certain obscure studies like the More-Frey phase ratio experiment so meticulously documented, while a series of investigations like the Chicago series on the perception of relative motion escape mention, although they develop aspects of the motion problem quite overlooked by Gestalt theory. Occasionally, an interpretation is placed upon an experimenter's results which is in direct opposition to the interpretation made by the experimenter himself, without recognition of the discrepancy. The reinterpretation cannot be objected to, certainly, but it might help to label it as such. An example is the Ewert repetition of the Stratton experiment on prism inversion of the retinal image. Boring's interpretation is probably more correct than Ewert's, but the controversial issues raised by Ewert's interpretation are overlooked. The chapter on organic sensibility is one of the best in the book, due to the author's own intimate personal contact with the field through his researches; the discussion of hunger is a real contribution toward clarity in a topic which has been sadly muddled. The final interpretation cannot fail to delight a functionalist's heart. A mention of Bash's study, involving sectioning of the vagus nerve and splanchnics, would have been quite apropos; for though Bash, in 1939, was repeating Sedillot, 1829, nevertheless the proof that the hunger drive was not eliminated was based on much more objectively demonstrable grounds.

What features of the book are most calculated to enhance its usefulness? First of all, its easy style; since the reviewer doubts the author's expectation that it will be used chiefly as a reference work. It is really readable. Second, the careful documentation both through the dates attached to names, and through the extensive notes at the end of each chapter. Third, the fact that the orientation is around topics rather than around historical periods, which makes it a simple matter to work up the history of a particular subject, such as after-images, without wading through quantities of irrelevant material. Fourth, the historical spiral appearing on the cover page which permits those who prefer a continuous linear view of history to trace the epochs from 1600 to date. Fifth, the illustrations and graphs which have been generously distributed throughout, since it is recognized that often the discovery of some new device or instrument is responsible for the rapid development of some topic which might otherwise have been buried in obscurity.

What conclusions can be drawn from the book regarding the probable future directions of development of this historically dominating field of

sensation and perception in psychology? Some of the major battles of the past have ended, such as the one over sensations of innervation, because further neurological evidence has settled the question; others, such as the problem of "specificity of nerve energies," are approaching final clarification, for a similar reason. Also, controversies like those waged over the rival color theories are dying out, first because work such as Hecht's has made them obsolete, second, because of the growing realization of the hopelessness of working out a comprehensive color theory until many more neurological facts are known. Along with the submerging of the nativistic-empiricistic controversy has gone the problem of local sign, the horopter, etc. Nativism has merged into Gestalt; empiricism into behavioristic operationism. On the other hand, because of new evidence, some ancient controversies have suddenly burst into the open again, such as the relative merits of resonance versus frequency theories of hearing; or the quantitative versus the qualitative theory of differentiation of cutaneous sensations. But what of the future? Two divergent trends seem to be emerging: on the one hand, at the hands of the Gestaltists, phenomenology seems to be taking a new direction, away from elements to dynamic patterns, and to a psycho-neural isomorphism in which brain process is made the exact parallel of phenomenal field; on the other hand, empiricism is seeking an entirely different sort of isomorphism, at the hands of Operational Behaviorism, in which relations of a causal order are sought between neural events and other physiological events involved in the description of experience.

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GRAY, J. STANLEY, Editor, and others. *Psychology in use*, a textbook in applied psychology. New York: American Book Company, 1941. Pp. xii+663.

This volume, composed of an introduction and 12 chapters dealing with various aspects of applied psychology, has been planned to meet the very real need for a textbook in this field. The range of material is wide, with chapters on the following topics: basic principles, problems of every day life, clinical practice, student personnel work, psychology in home life, education, industry, business, law and criminology, medicine, influencing other people, and social reform.

The estimate of whether the purpose of the book has been fulfilled will depend in large part on what one considers the need to be. The present volume provides an exceptionally readable survey of many of the areas in which psychology has application and it is safe to say that the average undergraduate will be fascinated by the material presented. The question may be raised, however, whether the time has not arrived in the development of the science of behavior when even beginning courses should be expected to train students in the techniques of the science, as do courses in applied aspects of chemistry, physics and biology, or whether we should continue to present courses which do little more than introduce the student to an interesting subject, without giving him any technical proficiency in the field. "*Psychology in Use*" is well adapted to elicit

interest, for it is an excellent exhibit of the most recent results of the psychologist's efforts and desires to be useful, but on completion of such a text the student will have gained little specific knowledge of the methodologies of applied psychology. Perhaps we must still count on using beginning courses to win converts who will go to graduate school for their techniques. Wartime demands make this question a serious one.

This is not to say that the book fails to mention important psychological instruments, such as tests, which are described in four or five different chapters. This scattering of information on testing makes it somewhat difficult to obtain a systematic view of the material, and some will feel that this weakens the presentation. On the other hand, this result is incident to the fact that the chapters are focussed on practical fields as they are actually encountered, a method which does have the advantage of tying in with student interest and set toward these practical problems.

Actually there is surprisingly little overlapping of content in the various chapters of the book and in no other case is there such scattered reference to a topic as has been indicated in the discussion of testing. This has been accomplished by parcelling out, sometimes in a rather arbitrary manner, certain topics to certain writers for discussion. Rating scales, for example, are discussed principally in a consideration of Psychology in Industry, though they are equally important in other connections and could be brought in elsewhere.

In general, the book escapes the triviality that is often a feature of "practical" psychologies, though the chapters are somewhat uneven in this respect. This is to be expected since some fields of application are actually richer in available material than others. This difficulty is apparent in the chapters on "How Is Psychology Used in Everyday Life" and "How Is Psychology Used in Influencing People." The first of these is poorly integrated, being composed of a hodge-podge of topics such as psychology and the motorist, accidents, sleep and relaxation, and the effects of age on behavior. Moreover, some of the content of this chapter is of doubtful relevance. For example, it is hardly a discovery of the science of psychology nor an impressive fact that, "The inevitable presence of soap suds" in bath tubs is part of the explanation of why tubs are slippery.

Many of us have been negatively conditioned to discussions about influencing people. The chapter by that name is superior to most comments on the topic. It is difficult to understand, however, why discussions of this sort so often assume that we are interested only in finding out how to lead people blindly by means of devious techniques of suggestion and flattery. Many people, including some educators, are equally concerned about stimulating critical thinking. Moreover, a great deal in a chapter like this never gets beyond the lore of the man-in-the-street and had better be omitted from a volume until it can be cast into a clearer form. When, for example, the author says, "In most human behavior intelligence is not involved" and emotions are the chief determinants of belief and action, he is not speaking with accuracy and it is doubtful that successful control of behavior lies this way. The discussion that follows emphasizes the importance of appealing to the desire for social approval, for money,

etc. This is sound advice but why use an appeal to these motives as synonymous with an appeal to emotion? And why imply that behavior is either intelligent or emotional? Even when response is an unreasoning and automatic reaction to suggestions, it is not necessarily emotional. It may involve merely habitual responses that run off so smoothly that there is no occasion for emotion. Furthermore, acting on motives is not incompatible with using intelligence to reach the goal. But the problem is complex and should not be labored. It is merely questioned that much is gained by superficial treatment of such questions.

HELEN PEAK.

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BENDER, I. E., IMUS, H. A., ROTHNEY, J. W. M., KEMPLE, C., and ENGLAND, M. R. Motivation and visual factors; individual studies of college students. Hanover, N. H.: Dartmouth College Publications, 1942. Pp. xix+369.

An earlier study from the Dartmouth Clinic, *An Evaluation of Visual Factors in Reading*, showed in group comparisons that visual defects had no relationship to severity of the symptoms nor to performance on tests and grades. The present study represents a further and more detailed analysis of data for some of these same students. In the present study, however, an attempt is made through intensive case studies to analyze the role of the visual defect in the total personality pattern of the individual. The analysis thus turns from a study of the characteristics of persons in general to a study of the lawful tendencies within individual personality patterns.

In the original study, 636 students in the Dartmouth College Class of 1940 were studied. In the present report 124 of these were selected for intensive study as representing various types of visual and educational conditions. However, the bulk of the report is devoted to case studies of only twenty students. Data are given indicating that these subgroups were representative of the whole class in intelligence, reading and grades; other comparative data are not provided. It must be noted (as the author does point out), however, that Dartmouth students are not a representative group even in their distribution of visual disabilities. Only 2 of the 124 cases were rated by an experienced oculist as representing less than a middle rating on a six point scale of severity of eye difficulty. Further, these tend to be young men from a select social-economic background who are living in the academic atmosphere. Persons from other cultures and in other occupations would obviously have other personality patterns and visual demands. The authors have been careful to limit their interpretations to college students with minor visual disabilities.

The following procedures were used to obtain data on psychological factors and motivation: interviews, autobiographies, projective techniques, behavior descriptions by others, ratings by associates, objective tests, and psychological questionnaires. Complete visual examinations were given. These data were analyzed and checked by at least two workers and written up as psycho-portraits (average length of twelve pages).

On the basis of their evidence the authors feel that they have been unable to determine the effect of visual defects on motivation. In every case, the visual defect was so deeply imbedded within the total personality that its psychological significance was not clear. But it does seem clear to them that the motivational pattern of the individual has determined his reaction to the visual defect. Eye defects cannot be considered as separate and specific but must be considered in terms of the orientation of the total personality. Students with similar eye deficiencies had no more similar personality patterns than individuals with different eye conditions. The correction of an eye defect had no appreciable effect on college achievement. The expressed severity of symptoms was more closely related to the student's motivation pattern than to the degree of his visual defect.

This is an excellent and challenging study. It departs from the usual techniques of treating the results of each test separately and uses descriptive analyses of the personality patterns of each individual. This study suggests that an appreciation of the total personality pattern is necessary before interpretation of a test score may be made.

Criticisms to be made are minor. (1) This study emphasizes the manner in which a student views his own defects. It seems to the reviewer that too many clinical studies deal only with such introspective data. Such material is important, but it is also about time that a form of behaviorism hit clinical psychology. Possibly visual defects have an effect on behavior of which the student is not aware; studies of observable behavior rather than mental reactions might show such a relationship. (2) The case studies seem well done, but the 258 pages of case studies with their mass of detail and complex relationships may be as difficult to assimilate as raw data through normal reading techniques. Yet any one case study seems almost too concise when a thorough study is made of it. This difficulty, however, is inherent in the approach used. (3) A correlation of .613 (pp. 35-36) between measures of aniseikonia at distant and near vision is interpreted as follows: "translated into terms of prediction, the coefficient indicates the presence of both distant and near aniseikonia in approximately twenty per cent of the cases." While twenty per cent of the cases in this group may have had both defects, one cannot necessarily *predict* that an equal per cent of cases will have two defects when such a correlation is obtained with another group, *i.e.*, one in which visual efficiency ranges from average to extremely deficiency vision.

FRANCIS P. ROBINSON.

*Ohio State University.*



### BOOKS AND MATERIALS RECEIVED

FRY, C. C. (with the collaboration of EDNA G. ROSTOW). Mental health in college. New York: Commonwealth Fund, 1942. Pp. xiv+365.

LOWINGER, A. The methodology of Pierre Duhem. New York: Columbia University Press, 1941. Pp. 184.

MOORE, H. Psychology for business and industry. (2nd Ed.) New York: McGraw-Hill, 1942. Pp. xiv+526.

STERN, EDITH M. (with S. W. HAMILTON). Mental illness: a guide for the family. New York: Commonwealth Fund, 1942. Pp. xvii+134.

VARNUM, W. C. Psychology in everyday life. New York: McGraw-Hill, 1942. Pp. xiv+490.

WOLF, ANNA W. Our children face war. Boston: Houghton Mifflin, 1942. Pp. vii+214.

———. Report of the Eleventh annual conference on delinquency prevention (sponsored by The Division for Delinquency Prevention in coöperation with the Big Brothers and Sisters Association of Illinois). Illinois: Department of Public Welfare, 1942. Pp. xviii+222.

## NOTES AND NEWS

CARL CAMPBELL BRIGHAM, professor of psychology, Princeton University, died, January 24, at the age of fifty-two years. He had served Princeton as instructor (1916-17), assistant professor (1920-24), associate professor (1924-27), and professor since 1928. He was also associate professor of psychology at Cooper Union, New York City, a member of the Committee on the Classification of Military Personnel, and research secretary of the College Entrance Examination Board.

ELIZABETH EVANS LORD, psychologist of the Children's Hospital of Boston, Mass., died, January 10th.

The Illinois Association for Applied Psychology is the new name for the group formerly known as the Illinois Society of Consulting Psychologists. The change is one in name only and the purposes, activities, and membership requirements of the group remain the same. The officers for the year 1942-43 are: *President*, ANDREW W. BROWN; *Vice-president*, ARTHUR W. KORNHAUSER; *Secretary-treasurer*, MILTON A. SAFFIR; *Members-at-large of the executive committee*, ADAM R. GILLILAND, MELVIN S. HATTWICK, and HELEN L. KOCH. The Association issues a monthly mimeographed News Letter which attempts both to summarize developments in the applied field in the state of Illinois and to report Association activities. The mailing list of the News Letter has recently been opened to non-member psychologists in the hope of increasing the solidarity of the profession in the state. Any member or associate of the APA who would like to receive the News Letter will be placed on the mailing list, if he will send his name and address to the editor, DR. FRANCES A. MULLEN, Bureau of Child Study, Board of Education, Chicago.

JOHN A. COOPER, formerly psychologist at the Eastern Penitentiary, Philadelphia, has been appointed instructor in psychology, State Teachers College, Frostburg, Md.

IRVING LORGE, executive officer, division of psychology, Institute of Educational Research, Teachers College, Columbia University, has been named director of an exploratory study of the meanings of economic competence. The Consumer Education Study of the National Association of Secondary School Principals has joined with the Lincoln School of Teachers College in providing funds for the study.

FRANK N. FREEMAN, dean, School of Education, University of California, has been selected by J. P. Nourse, superintendent of schools, San Francisco, to direct a revision of the elementary-school curriculum. Dean Freeman will be assisted by "two specialists in education in universities and colleges and three principals of elementary schools."

LOUIS W. GELLERMAN, associate professor of education, Southern Illinois Normal University (Carbondale), has accepted an administrative post in the public schools of Seattle.

GEORGE KATONA, fellow of the John Simon Guggenheim Foundation for 1940-42 and lecturer of psychology at the New School for Social Research, New York, has been appointed Associate Director of the Committee on Price Control and Rationing of the University of Chicago. The Committee is organized under the auspices of the Cowles Commission for Research in Economics and the Price Conference of the National Bureau of Economic Research, for the purpose of conducting a field study into the effects of price control.

ROBERT T. ROCK, JR., and FRANK A. GELDARD, psychologists in the Office of the Air Surgeon, Army Air Forces, have been promoted from the rank of Major to that of Lieutenant Colonel.

CHARLES H. JUDD, professor emeritus of education, the University of Chicago, is serving as expert consultant to the War Department at the Army School for Special Service. The school has recently been moved from Fort Meade (Md.) to the campus of Washington and Lee University (Lexington, Va.), and the course of instruction extended from four to six weeks.

DRS. HELEN L. KOCH and HELEN SCHACTER are participating in an extensive program for the training of volunteer child care aides in the Chicago area. Two-hundred and fifty volunteers in four training centers are being trained.

An internship in psychology has been established at the University of Illinois College of Medicine, Department of Psychiatry. MISS EILEEN SINCLAIR, of Manitoba, Canada, is filling this position for three months. DR. IRENE C. SHERMAN will direct her work.

MISS MARJORIE BREMNER, formerly psychologist with the Bureau of Child Study of the Chicago Board of Education has completed her basic training with the WAVES and has recently been assigned to the Bureau of Naval Personnel in Washington.

Applications to the Committee for Research in Problems of Sex of the NRC for financial aid in the year beginning July 1, to support work on problems of sex and reproduction, should be received before April 1. They may be addressed to the Chairman, DR. ROBERT M. YERKES, Yale School of Medicine, New Haven, Conn. Although hormonal investigations continue to command the interest of the committee, preference, in accordance with current policy, will ordinarily be given to proposals for the investigation of neurological, psychobiological and behavioral problems of sex and reproduction.

Associates and Members are requested to interest qualified persons in affiliation with the American Psychological Association. Letters have

recently gone to Associates of five years' standing calling attention to the method and qualifications for transfer. The deadline for applications for Member is March 15, 1943, for consideration at the September, 1943, meeting. The deadline for applications for Associateship is July 1, 1943. Application blanks are available at most departmental offices or may be obtained directly from the Secretary, WILLARD C. OLSON, University of Michigan, Ann Arbor, Michigan.

*Books for Prisoners of War.* A request has been received from the War Prisoners' Aid of the Young Men's Christian Association for books in the field of psychology, to be sent to allied prisoners of war who are in Axis hands. In the prisoner of war camps, there are teachers and graduate students in psychology, as well as classes organized by the prisoners themselves; requests for psychology textbooks and treatises are therefore frequent, and their provision is a great lift to morale. War Prisoners' Aid has had success in getting books into the hands of prisoners of war and from time to time prepares shipments to be sent across the seas.

One difficulty in transmitting books is that before shipment the approval of the censor of each book has to be obtained. Hence, a general request for donations to which, without doubt, many psychologists would respond, would be of little value. The procedure now worked out is that of requesting each psychologist to make up a list of the books that he is willing to donate for this worthy purpose and send it to MR. PAUL B. ANDERSON, War Prisoners' Aid, World's Committee of the Y.M.C.A., 347 Madison Avenue, New York City, who will check the lists with the censor and write to the prospective donor informing him which books will be accepted. Books sent to prisoners of war may not have any marks or erasures in them and the date of publication may not be later than September 1, 1939.

It is hoped that many psychologists will respond to this appeal by sending books within their own field. There have been requests from prisoners of war for technical and professional books, as well as for more popular types of books which are made available through other than professional sources.

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